OBITUARY NOTICES

CHARLES EDWARD SPEARMAN, 1863–1945

With the death of Charles Edward Spearman on 17 September 1945, there passed away one of the most influential figures in mental science that this country has produced. Indeed, there can be little doubt that he must be regarded as one of those relatively few great innovators who have imparted to their chosen branch of knowledge a new impetus and a new direction, thus permanently modifying its nature, course and methods. Psychology has definitely altered its complexion as the result of Spearman's work. To become convinced of this it is only necessary to take up at haphazard almost any recent number of any journal of psychology and to find therein references (often on a large scale) to coefficients of correlation and other statistical devices, the introduction of which into psychology was due either to Spearman himself or to the general interest in the subject that he aroused among contemporary workers.

But outstanding as was Spearman's influence in this direction, his real greatness lies also in the fact that he was fully conscious not only of the advantage of statistics and of quantitative and experimental methods generally, but also of their limitations. He often told his students that they should look upon statistics as a good servant but a bad master, and he had little interest in the mere accumulation of quantitative data for their own sake or in the vague hope that something would come out of them. Though an enthusiast for the experimental method, an experiment to his mind should have an idea behind it, should be so arranged as to prove, disprove, or at least throw light on, some hypothesis. Granted that this was the case, he was capable of experiencing and of communicating to others (both personally and through his writings) something like a real glow of aesthetic appreciation in a good experiment. The present writer well remembers an occasion on which, just before one of his lectures (a time otherwise rather scrupulously devoted to final preparation), he hurried to another room to learn the outcome of one of Wohlgemuth's experiments on visual and verbal memorizing, or again the moment when, with a deep sigh of pleasant expectation, he sat down to study the huge table of correlations between Webb's estimates of character qualities and Lankes' tests of 'p'. Such enthusiasm was infectious and accounted in large measure for his success in attracting to his laboratory a long succession of research students who together formed one of the most influential schools of psychology.

Not that Spearman was a narrow sectarian, seeing good only in his own particular method of approach. On the contrary, as his lectures and writings testified, he was happy to avail himself of any data from whatever source that seemed to have a bearing on the problem with which he was at the moment concerned. In one place he even went so far as to describe his own school as 'a school to end schools'; perhaps an unduly optimistic estimate of the influence of his own viewpoint and persuasiveness (he elsewhere regretfully admits that he sometimes aroused unexpected hostility and opposition), but one which nevertheless had a realistic foundation in the circumstance that factor-analysis was capable of revealing the elements of truth and mutual agreement in many apparently conflicting doctrines concerning the make-up of the human mind. But though well aware

of the limitations and drawbacks of the embarrassingly numerous schools of psychology, he was yet convinced that at certain stages of development they might have great advantages as providing each a strong spearhead of advance in critical directions and as exploiting to the utmost new methods and new points of view. It was with this in mind that he tactfully but effectively guided each new student into the work that seemed to him as director of the laboratory to fit best into the general co-ordinated scheme of research that he had in mind—no mean achievement considering that his students came from all parts of the earth, that they were not all at first enamoured of statistical methods and that the prospects that he opened to them, though ultimately highly remunerative from the scientific standpoint, often in themselves involved a great deal of hard work of a by no means immediately alluring kind. Like a great military leader, he was able both to think out and co-ordinate the details of a vast and complicated campaign and to enlist the willing and enthusiastic co-operation of his subordinates in the execution of these details. In this respect he was the ideal director of a new department for the study of the still young science of experimental psychology at a time when both the department and the science had to justify themselves to those who were responsible for academic policy in this country. If, as was sometimes complained, he participated but little in the more purely social aspects of University life outside his own department, he made ample amends through the activities of his laboratory and its increasing prestige throughout the world.

Born in London in 1863, Spearman from his early days onwards had been interested in philosophy, though he kept this interest largely to himself. Unsatisfied alike by the solutions to the great philosophical problems offered by the literature and by his own efforts in the same direction, he decided to shelve these interests for a while by entering the Army. In so doing he was perhaps guided by the illustrious precedents of Plato and Descartes, though later on he bitterly regretted this step as 'the mistake of his life'. His spell of military service lasted indeed, for one reason or another, longer than he had originally intended, and included active service in the Burmese war, for which he was awarded a medal and two clasps. During this time, however, he became convinced that the key to the ultimate problems of life and being was to be found, if anywhere, only in psychology, and in consequence he turned eagerly to such psychological writers as were available to him. These were mostly followers of the associationist and hedonist tradition, his reaction to which was in turn 'intensely negative'. Hearing of the new method of approach inaugurated by Wundt, he resigned his commission in 1897, and, like so many other distinguished psychologists of his own and of the immediately preceding generation, went to study the experimental methods in the master's own laboratory at Leipzig.

His career was destined to be twice interrupted by further periods of military service, the first during the Boer war when he was Deputy Assistant Adjutant-General in Guernsey, the second during the first world war, when he served on the general staff of the Tyne defences. Towards the end of this latter war, however, he returned to civilian life and found what was probably a more useful sphere of activity in directing researches for the Navy and other Services—work which, in addition to its immediate value, undoubtedly contributed to the wider-scale introduction of psychological methods in this country in the second world war.

On completing his period of apprenticeship in Germany (during which he worked for a short time with Külpe at Würzburg and Müller at Göttingen as well as at Leipzig), and

on finally returning to England in 1907, Spearman took charge of the (at first) small department of experimental psychology at University College, London, which had been established shortly before under the direction of McDougall. Here he was destined to remain (except for his last short term of military service) until 1931, being made Grote Professor of Mind and Logic in 1911, in succession to Carveth Read, and Professor of Psychology in 1928. After his retirement from University College, he became Emeritus Professor and continued to be active in a variety of ways, spending some considerable time in America, where he played a prominent part in designing the great Chicago experiment on 'unitary differential traits' (in which 94 tests were applied to 1100 subjects), and also visiting Egypt, where he undertook a spell of teaching at the University of Cairo. During the second world war he was Honorary Psychological Adviser at Chesterfield. In the course of his career Spearman received many honours from scientific and academic bodies at home and abroad. He was a Fellow of the Royal Society, an Honorary LL.D. of Wittenberg, a Member of the Deutsche Akademie der Naturforscher and a Foreign Associate of the National Academy of Sciences of the U.S.A. He was the second President of the British Psychological Society (C. S. Myers being the first), his period of office—under the three-year rule then in force—running from 1923 to 1926, while a little later he became an Honorary Member of the Society.

Apart from his minor interests, which were numerous, Spearman's main lines of endeavour lay in two directions, which find expression in his two great books, *The Nature of Intelligence and the Principles of Cognition* and *The Abilities of Man* respectively, though he eventually found to his great joy that the two streams ran together in a way that produced mutual reinforcement and illumination. The first stream had its source in his early reading of the associationist literature. Inadequate as the associationist account of mental functioning had in general seemed to him, J. S. Mill's conception of science as founded upon laws had made a deep impression. This, coupled with William James' declaration that for want of laws psychology was 'no science' but 'only the hope of a science', sent Spearman searching for a set of laws that (unlike those *ad hoc* and unsystematic generalizations he had found here and there 'peppered over the pages of psychology') would provide a fundamental and exhaustive system. These he found in the three qualitative and five quantitative laws so lucidly expounded in *The Nature of Intelligence* and so suggestively applied to aesthetics and other fields in his smaller volume on *Creative Mind*. As the title of this latter work implies, the essential feature of the first three laws was that they were noegenetic, that they accounted for the processes by which *new* products could enter the mind—a matter in which they differed fundamentally from mere reproductive or associative processes (which were comprised in the second of Spearman's quantitative laws), a matter too which, as it seemed to him, had been conspicuously and disastrously overlooked by the associationists.

Of the three noegenetic laws it was that of the eduction of relations which was first formulated. Here, as Spearman admits, there were many sign-posts provided by earlier writers, though the road to which they pointed had, he thought, been most inadequately explored and its true significance never fully realized. The second law, that of the eduction of correlates, was more peculiarly his own. Its discovery was indeed, as F. Aveling (perhaps the closest to him, in this part of his thought, of all contemporary psychologists) once put it, 'his most characteristic stroke of genius'. In his autobiographical sketch in *Psychology in Autobiography* Spearman tells us how he had for some time been conscious

of the inadequacy of the law of relations, in spite of its great importance, to account for all cases of noegenesis; there was clearly something still missing, though 'for the life of him' he could not discover what. Then one day, while he was discussing the problems of submarine warfare with a Naval Officer in a railway carriage, the long awaited 'brain wave' arrived. His naval friend had insisted that in fighting under water mere rehearsal of anticipated actions or emergencies was often insufficient, as it frequently happened that some altogether new and unforeseen line of action was required. As Spearman interpreted it, this meant that old relations applied to a new situation must be, and actually were, capable of giving rise to a new plan of behaviour, and the process of generating this new plan was what he subsequently called the educing of a correlate—a process which, as he soon found, exemplified a second law of noegenesis no less important than the first. Even this discovery, however, provided no completely full and satisfactory account of noegenesis. The two laws revealed different ways in which the mind can pass from two elements already given to a third and new one (in one case a relation, in the other a correlate). But some explanation was still required of the elements thus assumed to be already given. This he found in his third law of the 'apprehension of experience'. As Spearman freely admitted, we have still very much to learn about the operation of this third law, for instance its relation to 'sensation', to 'intelligence' and to the 'unconscious'. But with the formulation of the three laws he became convinced that, as regards the generation of new experience, the whole ground was covered. Here at least was a great step towards the achievement of that system of laws of which he was in search—a realization which, he tells us, was the most emotional event in his whole intellectual history. And, indeed, after this the formulation of the remaining 'quantitative' laws proved a relatively easy matter, much help being forthcoming from the theories and experimental data already available in psychological literature, so that the whole system of his principles of cognition could be presented in lecture form in 1920 and in *The Nature of Intelligence* three years later.

Though to Spearman this struggle with the problem of laws was the 'main current' of his life work, there can be little doubt that to the majority of his contemporaries his second stream of endeavour, that concerned with mental measurement, with tests and correlations, was even more interesting and significant. The demonstration of the use to psychology of correlation coefficients, the methods of correcting and refining the measures afforded by such coefficients, the development of the theory of '*g*' and '*s*' and the various statistical devices used to this end, the gradual broadening of Spearman's original approach to the whole concept and the various methods of factor-analysis—these are matters too well known to readers of this *Journal* to need stressing here. As already indicated, they have given to much of psychology an entirely new complexion. The age-old notions of faculties and types, which have created such confusion, but which show no signs of being abandoned—probably because they are ultimately indispensable for purposes of classification and of ordered thinking on the subject of human variability—are at last becoming amenable to scientific control. True, the process is proving more difficult and complicated than might perhaps at first have been hoped. Many of the 'factors' so far isolated seem only to appear with the use of certain kinds of test, with certain populations or under certain experimental conditions; and it is sometimes by no means easy to give a satisfying psychological interpretation of the statistical results. Apart from the very significant exception presented by '*g*', it looks at the moment as though much further large-scale

work will be needed before we shall be able to separate with certainty the more generally present factors corresponding to fundamental 'powers' or aspects of the human mind, from others which only occasionally appear and which would seem therefore to be more in the nature of artefacts. But the tools for carrying out this work are there (though they will doubtless be improved) and, granted adequate co-operation between a sufficiently large body of psychologists, there is little doubt that the problems involved will eventually be solved, so that one day we shall possess a scientifically founded inventory of all the major human faculties, and scientific criteria for distinguishing the true nature and diversity of human types, thus transforming two long-cherished dreams into realities. And it is as a result of Spearman's pioneering efforts that we can now look forward with reasonable confidence to such a happy consummation.

As regards the history of Spearman's own contributions in this field (contributions which, as he himself constantly stressed, can hardly be separated from those of his pupils and fellow-workers whom he so generously inspired) we may perhaps distinguish some six events which he regarded as specially significant and which gave him a special satisfaction. These were:

(1) The provision through the doctrine of '*g*' of a firm theoretical foundation for Binet's 'hotchpot' procedure in the selection of tests, a procedure which, though it worked well in practice, was grievously out of harmony with the then current interpretation of intelligence in terms of one or other sort of 'faculty'. In this matter Spearman's strongly felt need for establishing a harmony between theory and practice was the means of obtaining a greatly deepened insight into the whole significance of 'mental tests'.

(2) Closely connected with this was the successful bridging of the gulf between general psychology and the psychology of individual differences. Thanks to Spearman's work, the latter no longer stands alone, as it did in the early years of this century, as a separate discipline, fascinating enough in its own way and perhaps also of much practical importance, but largely divorced from the great body of psychology as taught in the chief text books from Herbart to Stout and even later.

(3) His establishment of the 'two-factor' theory, whereby the abilities involved in any task could, he suggested, be put into two categories, '*g*' and '*s*'. We can readily pardon him if, in his initial enthusiasm for this discovery, he failed perhaps at first sufficiently to emphasize that the said dichotomy (at least in the form in which he first expressed it) only holds good with a certain selection of tests, a failure which he subsequently was willing to rectify with his increasing recognition of the importance of 'broad factors'.

(4) The formulation by Lashley of the principles of 'equipotentiality' and 'mass function' of many parts of the brain, based on the finding in experiments with rats that in many cases the impairment of function was directly proportional to the loss of cerebral substance. This finding seemed to Spearman to fit in admirably with his own doctrine of '*g*'.

(5) The discovery of a similar harmony between the doctrines of '*g*' and '*s*' on the one hand and the findings in experiments in 'formal training' on the other. These results were in general quite irreconcilable with the prevalent educational notions concerning faculties, but were readily explicable on the assumption that '*g*' could not be improved by exercise and that training was effective only through the formation of certain habits or associations, which, varied in scope and nature though they might be, all came under the general law of retentivity. On the whole moreover these habits are surprisingly specific and to that extent harmonize excellently with the notion of '*s*'. The results obtained by the 'static'

approach through tests were thus corroborated by the more 'dynamic' approach through experiments on training.

(6) Most gratifying of all perhaps in Spearman's own view was the finding that, considered qualitatively, '*g*' manifested itself only in the noegenetic processes (but at the same time equally in all of them, in so far at least as they fell into the categories of relation and correlate eduction), and that therefore the '*g*' saturation of any test or task was directly proportional to the extent that it demanded noegenesis (as distinct for instance from retentivity or from sensory or motor capacity). It was here that Spearman's own two great streams of work and interest flowed together, and to a mind such as his, which was always seeking to co-ordinate theory with practice, this was a source of quite especial satisfaction.

But, in spite of these numerous ways in which Spearman was able during his lifetime to see psychology transformed from the mere 'hope of a science' into a discipline based on strictly scientific principles and methods, he was yet to the end vividly conscious of the little that had so far been achieved in this direction as compared with the vast expenditure of thought upon the subject by able minds throughout the centuries. His last great work *Psychology Down the Ages*, which is devoted to a unique historical survey of the chief psychological doctrines from the Greek thinkers onwards, is well calculated to bring the reader to Spearman's own conviction that sure and permanent advance can be made, not by unaided thought or intuition, however painstaking or brilliant, but only by the methods that have been successful in the other sciences, i.e. meticulous observation and experiment, the provisional establishment of theories to account for the data thus obtained, and in turn the testing of these theories by further controlled experimentation. Spearman has himself done so much in this direction that all later generations of psychologists will be in debt to him. In spite of the temporary set-back due to the world war, others in ever-increasing numbers are eagerly following up the trails that he has blazed: and in this assuredly lies the true reward of a great and ardent pioneer.

J. C. Flugel

GEORGES DUMAS, 1866–1946

Georges Dumas, one of the masters of French psychology, died in February 1946 at the age of eighty, in the little village of Lédignan where he was born. During the occupation of France, letters from him came through to me irregularly. The correspondence ceased at the liberation, and I saw in this the distressing signs of Dumas's decline in strength.

Dumas was both doctor of letters and doctor of medicine. He left an imposing body of scientific work which gives evidence of the wide range of his interests and of his great learning in the domain of psycho-physics.

He was made director of the laboratory of psychopathology at the clinic for mental diseases at Sainte-Anne in 1896; Professor of Experimental Psychology in the University of Paris in 1912; Member of the Academy of Medicine in 1926. He was the author of a long list of works, the first of which was *Tolstoi et la philosophie de l'amour*, published in 1893, and the last *Le Nouveau Traité de Psychologie*, in ten volumes, begun in 1930, which monumental work appeared under his direction. In the intervening period, he published *La tristesse et la joie* (1900), *Psychologie de deux messies positivistes, Saint-Simon et Auguste Comte* (1905), *Le sourire, psychologie et physiologie* (1906), *Névroses et psychoses de guerre chez les Austro-Allemands* (1918), *Troubles mentaux et troubles nerveux de guerre* (1920).

The learning of Georges Dumas was neither heavy nor austere—it was French. In *Quelques réflexions sur la singularité d'être Français*, Roger Vailland maintains that the Frenchman, unlike the provincial, is always at his ease. Far from showing a lack of depth, this lightness of touch indicates a high degree of power, in the sense in which one talks of the power of an engine. This 'easiness' means that he is never forced to strain himself to the uttermost. The racehorse delights in obstacles; and so the Frenchman delights in difficulties and treats them with irony.

If the Englishman excels in his sense of humour, the Frenchman relies on his geniality; this is a sign of that French wit which implies a rare gift of flexibility and of secret energy, as Sainte-Beuve has pointed out. Dr A. Borel has said that all who knew Georges Dumas will keep the memory of his face with its accentuated Latin features, of his amused smile, of his sparkling talk. 'A dazzling conversationalist, he recounted with smiling humour a hundred memories of his long career. What anecdotes and what stories he told in his mellow voice with its southern tones! He outlined with equally humorous expression both the diverse literary, philosophical and artistic movements he had known, and also the amusing gallery of personalities whom he had met. His penetrating Latin good sense enabled him to criticize kindly both books and men, for though a complete master of irony, he softened its thrusts by an expression of kindliness which never seemed to leave him.'

Georges Dumas could hold forth on science, but without arrogance or superstition. He was ready to revise the theories he had formerly expounded. Thus he said at first, in his study of the smile, that the voluntary imitation of our reflexes and spontaneous reactions was one of the most general principles of expression. But later he revised this opinion, as the result of observations made on those born blind, and recognized the influence of a general systematization on the sum of our reactions, and the necessity of studying expressions not belonging to the normal pattern, such as occur particularly in the dissociated, the excited, the melancholic and the manic.

Let us also add that Dumas was, and never ceased to be, a psychologist. This dictum, which may appear trite, has some meaning if one takes into account the fact that nowadays many studies are included under the title of psychology which are psychological in name only. One could not say of Georges Dumas what Dr William Brown of Oxford wrote about Pavlov. William Brown pointed out that Pavlov's work on conditioned reflexes was greatly limited by the fact that he refused to take psychological concepts into consideration in any way in formulating his theories as to the nature of conditioned reflexes; thus these theories were inevitably purely physiological and mechanistic. Georges Dumas always dissociated himself from any such error.

Moreover, although Georges Dumas admitted that the different methods of investigation proposed by psychologists are of unequal value, from the point of view of the knowledge of psychological facts, he did not exclude any of them as a foregone conclusion. Hence his definition of psychology could rally the unanimity of the collaborators of his *Treaty*. Dumas conceived psychology as a science, in which introspection plays an essential and preliminary part for the description of facts and the analysis of mental mechanisms, in which biology and sociology investigate the biological roots and the social evolution of psychical functions, and in which psychopathology and neuropathology contribute to a great extent to its development by analysing mental and motor disturbances as well as nervous influences.

L. Litwinski

SOME QUALITATIVE OBSERVATIONS ON VERBAL MEMORY IN CASES OF CEREBRAL LESION

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I. *Introduction* (p. 8). II. *Test procedures* (pp. 8–9). III. *Case material* (p. 9). IV. *Results* (pp. 9–16): (1) *Auditory-verbal memory span* (pp. 9–10); (2) *Rote learning* (pp. 10–13); (3) *Substance recall* (pp. 13–16). V. *Discussion* (pp. 16–18). VI. *Summary* (p. 18). *References* (p. 19).

I. Introduction

This paper has grown out of a practical project to explore, select and develop suitable test techniques for assessing memory impairment in routine neuropsychiatric examination. A preliminary account of this work, in which experience with three types of learning test was reviewed, has been given in an earlier communication (Zangwill, 1943). These tests comprised: (1) a modified form of the Wechsler digit-test (Wechsler, 1941); (2) a sentence-learning test adapted from an item in Babcock's deterioration scale (Babcock, 1930); and (3) the Rey-Davis performance test (Rey, 1934). It was concluded that all three tests had clinical value but that special care was indicated in assessing cases with history of language disorder (dysphasia). This precaution is made necessary by the fact that cases with residual dysphasic signs commonly show a circumscribed impairment of verbal learning and their performance on the first two tests in consequence gives no reliable index of learning capacity in non-verbal fields. More recent experience with these tests has convinced us that the language factor is of even greater significance than was at first supposed. It was therefore felt that fuller exploration of the whole field of language disorder in relation to memory was called for before specific test procedures could be advocated with any confidence for routine use. As a first step in this direction, the material given on memory tests by a large group of dysphasic cases referred for routine psychological examination was analysed with a view to defining the effects of the various types of focal language disorder on verbal learning and recall. A similar analysis was then made of the material given by cases of cerebral lesion without dysphasia, including a small number of cases with an outspoken disturbance of general memory (amnesic or Korsakow syndrome). The results of this inquiry suggested a number of differences in the performance of dysphasic and dysmnesic cases on the tests which would repay more concentrated study. Pending this, however, it is perhaps in place to communicate the principal results in broad outline, with illustrations chosen from the different types of organic case that have come under our observation. A full statistical analysis is a task for the future.

II. Test procedures

The results on three types of verbal test will be discussed. These comprise: (1) tests of immediate repetition (auditory-verbal memory span); (2) tests of rote learning; and (3) tests of reproducing short stories and kindred material (substance recall).

(1) *Memory span.* The first part of Wechsler's digit-test (Wechsler, 1941) has been used as a routine procedure. The material consists of seven pairs of digit groups comprising from three to nine digits. The subject repeats each set forwards after one hearing. The span is arbitrarily defined as the longest set of digits that can be recited without error under these conditions. Supplementary procedures used in certain

cases include the Stanford-Binet subtests, Form L, V.5, VIII.6, XI.4, XIII.2 and AA.7 (Terman & Merrill, 1937) and Babcock's progressive sentence repeating test (Babcock, 1930, test 22).

(2) *Rote learning.* The standard test was the simple learning procedure adapted by the writer (Zangwill, 1943) from Babcock's sentence repeating test. It reads:

"One thing a nation must have to become rich and great is a large secure supply of wood."

The above sentence is repeated alternately by the examiner and the subject until the latter has recited two consecutive word-perfect versions or has attempted ten trials without complete success. In special cases, a patient who failed to learn the sentence under these conditions was allowed further trials in which he himself read the sentence aloud before repeating it.

Supplementary tests used included the Healy-Bronner Auditory Verbal Memory test (Weisenburg & McBride, 1935). The material consists of a short story, of 82 words, which has not been standardized as a rote learning test but can easily be adapted to this purpose. The learning procedure was not rigidly defined, but a judicious combination of 'whole' and 'part' methods was generally employed.

(3) *Substance recall.* We used mainly the 'Cowboy' story, long familiar to clinical psychiatrists, in the form approved by the Brain Injuries Committee (Subcommittee on Psychological Tests) of the Medical Research Council in 1941. The story is as follows:

"A cowboy went to San Francisco with his dog which he left at a friend's while he went to buy a new suit of clothes. Dressed in his grand new suit, he came back to the dog, whistled to it, called it by name, and patted it. But the dog would have nothing to do with him in his new hat and coat and gave a mournful howl. Coaxing was of no avail, so the cowboy went away and put on his old suit. And then the dog immediately showed its wild joy on seeing its master as it thought he ought to be."

This story was recited once, slowly, by the examiner and immediately repeated by the subject in his own words. Warning was always given that recall would be demanded but in some cases later reproductions were requested without previous warning.

The Stanford-Binet subtests, Form L, X.3 ('Reading and Report') and SA.II.6 ('Reproducing Thought of Passage') were also used in special cases.

The results obtained will be evaluated for present purposes in purely qualitative terms and it must not be assumed that any conclusion necessarily possesses statistical validity.

III. CASE MATERIAL

The observations on which this paper is based have been selected from the accumulated records of some 200 neuropsychiatric cases referred to the psychologist for examination at the Brain Injuries Centre, Edinburgh. The majority were cases of traumatic brain injury, including penetrating wounds of the head and post-concussional conditions. The remainder were cases of cerebral disease (vascular lesions, brain tumours, degenerative conditions). Sixty of the cases showed dysphasic manifestations or had an established history of speech disorder associated with a focal lesion. Cases of pronounced memory disorder (amnesic or Korsakow syndrome) were relatively few and our observations relating to this condition in consequence include several references to the same cases. They comprise one case of post-meningitic amnesic syndrome, one case of amnesic syndrome associated with left frontal lobectomy, and several cases with more or less pronounced amnesic states following severe concussional head injury.

IV. RESULTS

(1) *Auditory-verbal memory span*

Our findings with the repeating tests strongly suggest that the memory span for verbal material is seldom, if ever, affected to an appreciable degree in cases of cerebral lesion without dysphasia or history of dysphasia. This is in keeping with Weisenburg & McBride's observation on the group of cases with right-sided cerebral lesions used as a control in their study of aphasia. These authors report that the control group came very close to the

normal on tests of the same general type as we have used. A particularly interesting point, further, is the fact that all our cases of amnesic syndrome succeeded very adequately on the repeating tests.

A case of *post-meningitic amnesic syndrome* showing a very gross disturbance of memory and learning in all fields could always repeat 8 digits forwards and reached a superior level (Score: 20) on Babcock's sentence repetition test. A case of *post-traumatic amnesic syndrome* with gross disturbance of memory and orientation could likewise repeat 8 digits and reproduced the material of the Stanford-Binet subtests L.XI.4, L.XIII.2 and L.AA.7 with only trivial errors.

These results confirm Wechsler's earlier findings in cases of alcoholic Korsakow psychosis (Wechsler, 1917) and show that immediate verbal memory is typically intact in amnesic conditions.

A very different picture was given by our cases of dysphasia and those who had recently recovered from an acute aphasic condition. In these cases, very poor immediate repetition was almost universal. This gross defect of repeating was often in sharp contrast to the excellent speech of the patient and was often noted in cases who gave little, if any, evidence of dysphasia on routine examination.

Cases of *nominal* (or amnesic) and *syntactical* (or jargon) aphasia appear exceptionally prone to fail on the repeating tests. In nominal aphasia, the mere act of repeating to request is apt to give the patient remarkable difficulty. Thus he may fail to repeat, when asked, a word which he has just uttered spontaneously. But in view of the fact that he can always recognize the required word when it is told him it is doubtful whether his inability to repeat it can properly be treated as an amnesic phenomenon. It would perhaps be more fittingly denoted by the term 'apraxia for repetition'. In cases of syntactical aphasia, the memory span is generally much reduced and may extend to only 3 or 4 digits. On sentence repeating tests, it is notable that the patient may lapse into jargon when attempting the longer sentences although no longer using jargon in his ordinary conversation.

The results obtained from cases of cerebral lesion without history of aphasia and with no disturbance of general memory are a little variable but significant impairment appears to be very rare. Where it is found, indeed, an emotional complication (psychoneurotic admixture) must always be considered. Thus poor and inconsistent performance on the digit test in non-dysphasic cases without gross memory disturbance very often indicates a functional condition (Wechsler, 1941; Zangwill, 1943). This point is occasionally helpful in the differential diagnosis of organic and psychogenic sequelae of head injury in the psychological sphere.

(2) *Rote learning*

We may preface this section with a short account of previous work on verbal learning in cases of cerebral lesion.

Isserlin (1923), who made much use of the Ebbinghaus methods during the last war, found considerable impairment of rote learning in a large proportion of his brain-injured material. He also noted that cases with history of aphasia in general show gross impairment on learning tests and claimed that this finding might have both diagnostic and practical significance. More recently, Conkey (1938) has emphasized the frequency of learning defects following head injury and claims that these defects may long outlast residual mental disabilities of other types. Learning in the chronic organic psychoses has been studied by many workers and some of the more important contributions have been summarized by Hunt (1936). In general, the upshot appears to be that some impairment of rote learning is exceptionally common in cases of cerebral lesion irrespective of the precise pathology and localization. The defect is, however, especially pronounced in cases of aphasia (see also Weisenburg & McBride, 1935).

Our general experience with learning tests confirms the view that some degree of impairment in this field is exceptionally common in cases of brain injury. We cannot offer

a reliable estimate of the proportion of our own material showing such a defect but a rough estimate of 80% is unlikely to prove excessive. We have likewise found that verbal learning is almost always gravely impaired in cases of dysphasia and noted in a large proportion of the cases that a relatively gross learning defect is quite consistent with excellent recovery of ordinary speech.

The standard test used in dysphasic cases was the Babcock sentence given as described above (§ II). Of the cases with manifest dysphasia or history of an aphasic condition only 5% were able to learn the sentence by heart in less than ten consecutive trials and many failed to repeat it correctly after a very much larger number of trials. Patients of this type were seldom able to give more than the first three or four words of the sentence on the first trial and their later versions were commonly incomplete or even fragmentary. It was also noted that performance often deteriorated on the later trials and the last repetition in a series of ten was commonly inferior to those given earlier ('terminal deterioration'). A number of these features are well brought out in the following records from cases with slight residual dysphasia:

A case with *residual expressional defects* who had made an excellent recovery from an acute aphasic condition and who could express himself admirably in conversation completely failed to learn the Babcock sentence in ten trials. The exceptionally poor quality of his reproduction is well shown by the following samples:

Repetition 1: One thing a nation...which....
Repetition 2: One thing a nation must have...is a large amount of security....
Repetition 3: One thing...is...amount of security and wood.
Repetition 6: One thing...a nation must have...is rich and poor...amount of wood.
Repetition 10: ...a nation must have....

These extracts from the complete record bring out clearly the halting and fragmentary repetition with very poor learning and terminal deterioration.

A case of *residual syntactical and nominal dysphasia* whose speech was fluent and on the whole accurate was unable to reproduce the test sentence verbatim after ten trials.

Repetition 1: One thing a nation must have...must be much supplied...(something about) wood.
Repetition 6: One thing a nation must have...must be rich and great...something rich....
Repetition 10: One thing a nation must have be rich and great...(something to have)...wood.

In this case we likewise note the very fragmentary repetitions with poor grammar and little learning. Examples showing these general features could be multiplied indefinitely from our dysphasic material.

It is by no means uncommon on this test to obtain frankly dysphasic responses from patients who show no disturbance of language on ordinary examination. The test technique can therefore be treated as a method of eliciting dysphasic symptoms in very mild cases of speech disorder. Their character, moreover, may give an indication of the type of dysphasia displayed and thereby assist clinically in localization of the lesion. Thus a tendency to dysarthria or 'telegram style' suggests a predominantly executive ('frontal') aphasia whereas word-amnesia, paraphasia and paragrammatism are generally associated with the receptive ('temporal') syndromes (cf. Pick, 1931). The latter features are well shown by the following cases of post-traumatic aphasia who had made virtually complete recoveries from the acute condition.

A case of *residual syntactical aphasia* made an excellent recovery from a severe jargon-aphasia and very seldom used wrong words in conversation. Yet he was quite unable to repeat the Babcock sentence and at once lapsed into jargon when he was asked to repeat even small sections of it. Thus he reproduced "a large secure supply of wood" as "a long secure lie of water". A case of this kind, although extreme, is not exceptional.

A second case of the same type showed a less extreme degree of paraphasia but pronounced difficulties in word-finding and grammar.

Repetition 1: One thing...that...we must have...stuff (What do you call it?).
Repetition 6: One thing that a nation must have...is...wood.
Repetition 10: One thing that a nation must have...to become a great...and patient...wood.

The following case of a *residual verbal dysphasic syndrome* shows very similar repeating and learning difficulties but one may comment on the absence of paraphasia.

Repetition 1: One thing a nation can do...is (Oh! I forget it!).
Repetition 5: One thing a nation must have...the wealth and....
Repetition 6: One thing a nation must have must come rich...to greatness...rich and greatness.
Repetition 7: ...large secure supply of wood (Ah! That's it!)—One thing of greatness and supply... cannot finish).
Repetition 10: One thing a nation must have...a great (No, another word!)...supply the wood.

A number of our dysphasic patients have shown a strong disposition to use *synonyms* in their sentence repetitions. The border-line between pathological paraphasia and the normal tendency in recall to use synonyms that do not appreciably alter the sense of the material is difficult to draw. Normal subjects, however, can nearly always correct a verbal substitution made on the first repetition of a short sentence in the course of the next two or three trials. In dysphasic cases, on the other hand, these substitutions commonly persist through a whole number of successive trials despite the patient's earnest effort to attain word-perfect recall.

Persistence of synonyms is well shown in the following record from a case of *verbal dysphasia*. This patient gave 'country' for 'nation' on Trials 1 to 5 and 'abundance' for 'supply' on Trials 3 to 10.

Repetition 1: One country must have to become great is...to have...wood.
Repetition 5: One thing a country must have to become rich and great is a large abundance of wood.
Repetition 7: One thing a nation must have to become rich and great is a large...(I can't get it! 'Abundance' or something).
Repetition 10: One thing a nation must have to become great is a large...abundance....

Some favourite synonyms in dysphasic sentence-repetition are 'safe' for 'secure', 'wealthy' for 'rich', and 'amount' for 'supply'.

Modified learning methods, e.g. allowing the patient to read the sentence himself, have in some cases given better results and in rare cases the difference in performance according as to whether the sentence is heard or read is striking. In general, however, learning is slow, laborious and seldom perfect whatever method is used.

Verbal learning was studied in some detail in two cases of *amnesic syndrome* in neither of whom was there the least trace of dysphasia. Both these cases, whose memory span was high, were able to repeat the Babcock sentence correctly after two or three trials. When tested with a longer passage, on the other hand, both showed a very severe defect of rote learning. Thus neither of the patients was able to learn the Healy-Bronner passage despite a variety of learning methods and repeated attempts on several consecutive days. One of the two patients attempted, in addition to a large number of repetitions of shorter parts of the passage, no less than 80 recitations of the whole passage without success. These results (together with very similar unpublished results which we have obtained in cases of alcoholic Korsakow psychosis) suggest that cases of amnesic syndrome are able to learn a short passage by heart with normal speed but fail grossly in attempting to learn longer or more complicated material. A more precise investigation of the limits of learning in amnesic conditions would form an interesting study.

Our results in non-dysphasic cases of cerebral lesion without gross retention defect are too irregular to justify inclusion here. Many cases are able to repeat the Babcock sentence correctly in less than ten repetitions but commonly give the impression that learning is a good deal slower than in the normal. A tendency to *stereotyped error* (the 'constant errors' of Brodmann, 1904) is often noted—more especially in cases of frontal-lobe pathology.

Stereotyped or constant errors are well shown in the following examples:

(1) A case of *left frontal lobe tumour*, tested post-operatively, gave the following version of Babcock's sentence on the third trial:

One thing a nation must have to become rich and great and have a large supply of wood.

This was repeated without any verbal change on the next four trials in the series, after which it was partly corrected.

(2) A second case of *left frontal lobe tumour* gave on the fourth trial:

One thing a nation must have to be large and great is a large supply of wood.

This was repeated unchanged on the following seven trials after which the test was broken off.

(3) A case of *post-traumatic frontal lobe atrophy* gave on the fifth trial:

One thing a nation must have to become rich and great is a large selection—a large secure selection—of wood.

This form of the sentence was also given on the next two trials, with an exactly similar pause and reduplication, and again on the final three trials, though now without the reduplication.

These stereotyped errors are sometimes so marked as to justify a diagnosis of organic intellectual impairment in doubtful cases. Their origin may be due in part to motor perseveration in the ordinary sense and in part to a specific failure to discriminate between what is said by the examiner and what the patient himself repeats. In some cases, indeed, the patient is obviously under the impression that he is repeating exactly what he has been told when in fact his reproduction is very different from the original.

(3) *Substance recall*

The results given by organic cases on tests of recalling short stories are in general too variable to serve any useful practical purpose and we have not therefore advocated the use of such tests for routine purposes (Zangwill, 1943). At the same time, the results given on these tests by certain types of case, more especially those with relatively marked dysphasic or dysmnesic symptoms, are often instructive and merit a brief review.

Let us consider firstly cases of *dysphasia*. These cases, as might well be expected, often present specific disabilities in the sphere of comprehension which gravely limit their capacity to take in and reproduce a short story. This is particularly marked in the types of case classified by Head (1926) as *semantic aphasia*. Semantic cases often fail to appreciate the full meaning or 'point' of a short story and cannot therefore reproduce it in coherent form. Apart from these, however, a great many cases of dysphasia of other types give surprisingly poor results on this type of memory test despite intact comprehension and fluent expression. A few examples may be given:

(1) A case of *residual expressive dysphasia* with perfect comprehension of speech and very little executive disability totally failed to reproduce the gist of the 'Cowboy' story although he claimed that he grasped the point of it as it was read to him. His reproduction was as follows:

A cowboy...went to...Fran Sancisco...and with his dog. The cowboy came back...and patted his dog on the shoulder...he was wild with joy...and leaped up...and...(unable to complete).

(2) A case of *residual verbal dysphasia* with intact oral comprehension and very adequate restitution of speech gave the following as his immediate reproduction of the 'Cowboy' story:

Cowboy from San Francisco...went with his dog (something before that)...went with his dog... changed his wearing apparel and the dog wouldn't have anything to do with him. He went back and put on his old clothing back again, and the dog immediately came and made a fuss of him. He whistled or something.

(3) A case of residual *post-traumatic syntactical and nominal dysphasia* who had largely recovered from his language disabilities so far as ordinary conversation was concerned gave the following version of the 'Cowboy' story:

Well a man went away to another place with his...not his dog, his shutt [*sic*]...he left it with another lad for the day and went away and bought some stuff [patient points vigorously to his own suit] and then he came back to...not his ship...where he left...the people that was with him. While he was away he bought this new stuff [again pointing to suit]. He went after it and tried to get it back, but it wouldn't come. He tried again.

The marked breakdown shown by the above cases on immediate recall, and the gross exaggeration of the dysphasic symptoms shown by the third case, strongly suggests that recall, under experimental conditions at least, is a more high-level and less automatic reaction than ordinary dialogue and is correspondingly more vulnerable.

We have found with few exceptions that cases with *slight* dysphasic handicaps succeed very much better on these substance recall tests than on the rote learning procedures described above.

This relative superiority of substance recall is strikingly shown in the (not uncommon) case of a patient who can give the gist of the Babcock sentence in his own words but who entirely fails to learn it by heart. Thus a young patient with expressional disabilities who was unable to reproduce more than six or seven words of the sentence on the learning trials gave the following version on free recall:

It is necessary for a nation to have a good supply of wood.

An interesting contrast between 'memory for words' and 'memory for sense' is likewise seen in the following case:

A case of *residual verbal dysphasia* attempted ten consecutive trials on the Babcock sentence without success. On the following day, he attempted a further ten trials under the same conditions. Although his repeating showed some improvement, he was still unable to reproduce the sentence verbatim, as the following extract shows:

Repetition 20: One thing a nation must have to become rich and great is a large...(I forget that part about the wood!).

His substance memory, on the other hand, was little, if at all, impaired. He reproduced the gist of the 'Cowboy' story in an acceptable manner and produced the following reproduction of the Stanford-Binet 'Reading and Report' test (Form L, X.3) after a single reading:

A fire at Manchester. There was £5000 damage. Seventeen families lost their homes. A fireman went to rescue a girl in bed and he was burned about his hands.

The above case is by no means exceptional, although the superiority of substance over rote recall is rather more marked than is the rule.

Our results in the few cases of *amnesic syndrome* that have come under our observation show interesting features. All these cases gave very sketchy reproductions of the 'Cowboy' story, of which the following is a good example:

A cowboy went to San Francisco to buy a suit of clothes. He left his dog with some friends. When he came back, the dog, instead of recognizing its master, wept and howled and ran away.

These patients often distort and elaborate the narrative in a striking manner and in some cases the resulting fabrications are developed progressively in repeated reproduc-

tions. Three examples drawn from our cases of acute head injury (post-traumatic Korsakow condition) may be cited:

(1) *Case* 1. *Acute Head Injury*:

(*a*) *Immediate Recall.* About a boy took a dog to San Francisco. I'm not sure whether he left it outside or took it in. Anyway, something happened to it and he was found guilty of being wrong somewhere or other.

(*b*) *Recall after* 2 *min.* The fellow supposed to take his dog to San Francisco and during the time he was there I'm not sure whether he was found out wrong for lack of behaviour in not taking the dog to digs or whether he was found out wrong himself.

(*c*) *Recall after* 5 *min.* This bloke took the dog to digs in San Francisco and I don't know whether he left him at the digs or what he did with him. I believe he took him away somewhere himself...and I believe he got pinched for something or other...I've missed something about his dress.

(2) *Case* 2. *Acute Head Injury:*

(*a*) *Immediate Recall.* An American fellow went to San Francisco to a friend's house to stay on a farm to see the dog there. The dog knew him and he expected the dog to know him again. But when the dog came to understand him, he didn't recognize him in the fresh clothes he'd had on for the journey. And the dog was unable to recognize him.

(*b*) *Recall after* 2 *days.* (Cue: 'Cowboy'.) A cowboy came along in a different suit and his animal, I think his dog, didn't recognize him. And he went to a ranch somewhere in South America, no, the south of North America (in the film district). He went to see someone on the ranch and they expected him to recognize him because his own animal was there. But his own animal wouldn't recognize him.

(*c*) *Recall after* 4 *days.* About a fellow going to the south part of North America to a ranch. When he came to the ranch he put on new clothes. And his animal wouldn't recognize him because he had new clothes on. I think it was his horse but I'm not sure.

(3) *Case* 3. *Post-traumatic Korsakow Syndrome:*

(*a*) *Immediate Recall.* The boss went to San Francisco to buy a suit. And he got one. When he came back the dog didn't take a liking to him. And let him know about it as best he could. [Suddenly] He thought he'd get changed; see about getting something else; no one liked it.

(*b*) *Recall after* 5 *min.* (Cue: 'Cowboy'.) A cowboy went to San Francisco and bought a suit of clothes. When he came back and put it on the dog barked and appeared not to know him. Which upset the man. He put on the old clothes and the dog was all right. A good natured man!

(*c*) *Recall after* 1 *day.* (Cue: 'Cowboy'.) About the man went to buy an overcoat or something and came back and showed it to his wife and his wife didn't like it. I think he went back and got another coat and this one suited them both. *Where was it*?—Sarcassy [*sic*] or somewhere. Some big city somewhere with an Asiatic name.

These records show far more omission, distortion and fabrication than is ever observed in comparable records from normal individuals obtained over relatively short periods of time. Case 1's reproductions show an interesting twist of the theme, introducing purely fictitious misconduct on the part of the cowboy. Case 2 shows less fabrication, but the point of the story is poorly explained throughout and there is a good deal of confused elaboration. The substitution of 'horse' for 'dog' on the third attempt is noteworthy. The reproductions of Case 3 (especially the second) are closer to the original but there is marked simplification and the substitution of 'wife' for 'dog' on the third recall is an interesting change. One may also call attention to the number of cases in which a cue was required before the patient could make any attempt at recall. This is very seldom necessary in normal (or for that matter dysphasic) subjects.

A further point of interest in connexion with the amnesic syndrome is the gross disorder of temporal reference or judgement with which it is associated (cf. van der Horst, 1932). This disorder may often be brought out very strikingly in connexion with test material.

Thus a gross case of amnesic syndrome we had occasion to study was told the Babcock sentence *two days* after he had successfully learnt it by heart. He recognized the sentence, but alleged he had read it in a paper two or three months ago in a speech by Mr Eden. This may be compared with a dysphasic patient who was told the sentence *eight months* after he had unsuccessfully tried to learn it by heart. This patient recognized the sentence and at once recalled the earlier test situation and his fruitless struggle to master the passage.

V. Discussion

Let us first consider the *auditory-verbal memory span* and its behaviour in cases of organic intracerebral pathology. As we have seen, the capacity to repeat verbatim short sequences of digits and the like is seldom, if ever, appreciably affected in cases of cerebral lesion without dysphasia (or history of dysphasia). Indeed, it may be remarkably well preserved in gross amnesic conditions such as Korsakow's psychosis. In dysphasic conditions, on the other hand, this capacity is as a rule severely impaired and may remain well below the norm in patients who in all major respects have made an excellent recovery from a transient (if severe) aphasic disorder. We may therefore conclude that a marked restriction of the memory-span is a clinical sign of a lesion involving the language mechanisms. We may further deduce that immediate verbal repetition in the normal depends in a decisive manner on this relatively circumscribed zone of the dominant hemisphere. In view of this, it is hardly surprising that the verbal memory span is unaffected in Korsakow conditions and that digit tests correlate poorly with other measures of general intelligence (cf. Wechsler, 1941).

We may now pass on to a brief discussion of *rote learning* and its disorders in organic cases. Our results, although largely qualitative, appear to correspond in essentials with the findings of Isserlin (1923) and other workers who have made use of more exact methods of study. Thus we have been led to conclude, as was Isserlin, that some learning deterioration in the verbal sphere is very common in cases of brain injury irrespective of the precise pathology or localization of the lesion. But this defect is exceptionally severe in two types of syndrome—the aphasic and the amnesic. We have shown that cases of dysphasia almost invariably learn verbal material in a halting and erratic manner and seldom succeed in reproducing the Babcock sentence without error under our admittedly liberal experimental conditions. We have likewise shown that cases of amnesic syndrome experience great difficulty in verbal learning although these cases, unlike the dysphasic, are generally able to learn the Babcock sentence in a more or less normal fashion. Their defect is apparent only when longer and more complex passages, such as the Healy-Bronner paragraph, are used as the test material. Patients of this type, though able to learn an individual sentence, are unable to learn a coherent narrative. Individual verbal responses can be readily acquired but *the patient is unable to organize them into a complete and coherent repetition habit.*[1] In dysphasic conditions, on the other hand, the patient is unable to learn even the sequence of words constituting a single short sentence. Stated more technically, *he is unable to acquire a novel verbal response identical in structure and meaning with the stimulus material.* The pattern of disability is thus very different. In the dysphasic case, we may fittingly speak of a *syntactical* learning defect, closely associated, no doubt, with the primary disorder of formulation and expression. In the amnesic case, on the other

[1] The term 'verbal response' as used in this context may be defined as a relevant and properly formulated sentence whose length does not appreciably exceed the scope of the memory span.

hand, there is no disorder of syntactical learning but a more generalized defect in the *serial organization* of the required responses. This is certainly not restricted to verbal material and would doubtless be found in any type of learning demanding the acquisition of a fixed series of reactions (e.g. in motor habits).

Our material obtained on tests of *substance recall* is far from easy to evaluate. This type of test is notoriously difficult to score and standardize in a manner that does adequate justice to the character of normal remembering. Those who advocate quantitative methods of scoring story material commonly ignore the fact that literal recall is the exception and not the rule (Bartlett, 1932). It is in consequence exceedingly difficult to define the accuracy of recall of any given story consistent with normal cerebral function. A great deal of omission and distortion is quite consistent with normal remembering in healthy individuals and the concept of *paramnesia*, which was originally defined by Kraepelin (1886) on the naïve assumption that recall to be normal must be accurate, is sadly in need of revision. At the same time, we believe that we are correct in holding that the majority of the records to which attention has been drawn above (§ IV(3)) betray their pathological origin. The material is, however, at present too slight to warrant further theoretical discussion.

An important psychological issue on which our results taken generally have some bearing is that of *the relationship of memory to habit*. This relationship has been the subject of heated controversy since Bergson's famous hypothesis of *les deux mémoires* (1911). Bergson's psychological followers, and most notably McDougall (1911), have treated rote learning as a biological function greatly inferior in status to recollection and recall and based on a very much simpler type of physiological mechanism. Their opponents, following Heymans (1913), have argued that these two aspects of memory differ only in degree of organization and may plausibly be held to depend on the same fundamental type of physiological disposition. This controversy, which has been reviewed by Oldfield & Zangwill (1938, 1942), cannot be further discussed here but it is in order to indicate briefly the manner in which the material discussed in this paper bears on it. Thus our experimental findings in cases of amnesic syndrome clearly show that both recollection and rote learning are equally impaired in this condition. This deterioration of memory in both its major aspects clearly conflicts with the expectations aroused by Bergson's analysis and his sharp distinction of habit and recall. Our findings in dysphasia, on the other hand, indicate that free recall of test material at least is often much superior to rote learning. In the limited field of language, therefore, our findings provide some support for Bergson's views. At the same time, it is important to bear in mind that the habit-forming disability in these cases is confined to language and there need be no impairment of the capacity to acquire serial habits in other (non-verbal) fields. The conclusion thus seems to be that recall and habit are equally affected in the amnesic syndrome but that in dysphasia there may be an impairment of habit formation in the verbal sphere without impairment of ordinary recall.

A few tentative conclusions of *practical import* can be drawn from this work. In the first place, it appears reasonably clear that immediate memory and sentence learning tests have value in detecting very slight degrees of language involvement associated with focal lesions of the speech areas. In the second place, the application of these tests in routine neuropsychiatric assessment has proved of value in calling attention to an aspect of mental impairment seldom detected on ordinary examination. And in the third place,

these tests used in conjunction with their non-verbal counterparts (e.g. the Rey-Davis test) have proved of very real help in advocating appropriate disposal of patients with psychological disabilities. This is especially true of cases in which the question of retraining a patient for a new occupation arises and must be decided on the basis of the available evidence regarding learning capacities. It is to be hoped that with better understanding of the nature and distribution of learning defects associated with cerebral lesions of various type and location there will be a corresponding advance in our methods of assessment and recommendations for vocational resettlement.

VI. Summary

1. Some qualitative observations on verbal memory in cases of cerebral lesion are described. These have reference to (*a*) the auditory-verbal memory span; (*b*) rote learning; and (*c*) recall of short stories (substance memory). Special attention is given to the results in cases with disorders of language and of memory respectively.

2. An impairment of memory span for verbal material and severe difficulties in rote learning were consistently found in cases with language disabilities associated with focal lesions of the speech areas. It is pointed out that these defects may long outlast the grosser symptoms of an acute aphasic disorder.

3. Cases of pronounced impairment of general memory (amnesic or Korsakow syndrome) were found to have intact verbal memory span but grossly impaired verbal learning. Substance recall was in general very defective in these cases and fabrication was often marked.

4. It is suggested that the defect of verbal learning in dysphasic cases is syntactical in nature and closely associated with the primary disorder of symbolic formulation and expression. In amnesic cases, on the other hand, it is argued that the defect is independent of language and due to a generalized inability to organize consecutive responses into a unitary habit.

5. It is pointed out that both rote and substance recall are equally affected in the amnesic syndrome but that the latter is often well preserved in dysphasic conditions. Some implications of this finding for the theory of memory are briefly considered.

6. The available evidence suggests that some impairment of verbal learning is very common in organic cases irrespective of the precise nature and localization of the lesion. A tendency to stereotyped error has been noted especially in cases with lesions of the frontal lobes.

7. The bearing of the results on practical problems of diagnosis, assessment and re-education in cases of brain injury is briefly indicated. Some suggestions for future research are also mentioned.

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AN EXAMINATION OF THE VARYING EFFECT OF CERTAIN STIMULI UPON THE ALPHA RHYTHM OF A SINGLE NORMAL INDIVIDUAL

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I. *Introduction* (p. 20). II. *Apparatus* (pp. 20–21). III. *Experimental method* (pp. 21–22). IV. *Measurement of results* (p. 22). V. *Results* (pp. 22–23). VI. *Discussion* (pp. 23–28). VII. *Summary and conclusions* (p. 28). *References* (p. 29).

I. Introduction

The majority of the research work carried out on the human electro-encephalogram (the E.E.G.) has been based on samples of the normal (or abnormal) population rather than on large numbers of examinations of a single individual. In nearly all these investigations of samples of the population mention is made of the large inter-individual variation found; and this is usually assumed to be due to differences in each subject, these differences remaining constant. Such variation can be equally well explained by assuming that each individual varies widely from hour to hour or day to day and that the sample happens to consist of individuals in different phases of their variations.

In single subject investigations experimenters have been concerned primarily with frequency, amplitude and location of foci of existing rhythms; and the constancy of response of the various rhythms (i.e. the alpha, beta, delta and theta rhythms) to different types of stimuli has been only a secondary problem. As far as the occipital alpha rhythm is concerned the general consensus of opinion seems to be that it is most readily suppressed by stimuli producing either a state of 'focused attention' or of 'autonomic tension' in the subject. These opinions are based largely on the results of the examination of samples of the population, and the factor of individual variation is stressed. The purpose of this experiment is systematically to examine the effect of various stimuli upon the alpha rhythm of a single subject, tested a large number of times, and thus to obtain a measure of the variation to be expected within a single individual. In a previous experiment (unpublished) the author carried out a small sample survey of normal individuals and certain of the findings will be compared with the results of the present experiment.

II. Apparatus

A single channel resistance capacity coupled amplifier having a balanced input circuit of the Tönnies type was used. For psychological reasons it was considered inadvisable to place the subject in a screened cage; and interference (almost entirely fifty cycle 'hum' pick-up from the mains wiring) was eliminated by careful balancing of the input circuit together with a source of 'hum' variable in amplitude and phase fed into the amplifier at a convenient stage. A special form of screened electrode was designed which reduced pick-up by a gratifying amount. These electrodes had to be carefully fitted, and moistened only where they were in contact with the head. Normal saline solution was used as the electrolyte. The output stage, connected as a cathode follower to provide a low impedance output, fed a Ferranti moving coil relay modified to act as a recording pen; while the actual record was taken on smoked paper passed round two standard kymograph drums arranged horizontally. These drums were driven from a small electric motor via a variable speed control. A Jacquet time marker was used to provide a simultaneous time trace, marking $\frac{1}{5}$ sec. A typical trace taken with this apparatus is shown in Fig. 1 (p. 23).

The equipment was designed and constructed at the Psychological Laboratory, University College, London, and has been used both there and at the writer's home.

Three electrodes were used, one 'indifferent' (earthed) and two 'live'. The 'indifferent' electrode was placed on the left mastoid process. One of the 'live' electrodes was placed on the right occipital process and the other about 2 in. above and just posterior to the right external meatus. Care was taken to avoid the occipital and superficial temporal arteries respectively. The electrodes were held in position by a rubber band encircling the head.

The subject reclined on a bed in a comfortable and relaxed position and kept his eyes closed unless otherwise ordered. No effort was made to control the lighting of the room (which was normal for the time of day) or any extraneous noises.

III. EXPERIMENTAL METHOD

(*a*) *Subject*

The subject (T.G.F.) is a male aged thirty having an I.Q. of 150 (Terman and Merrill, Form *M*) who has not suffered from any complaint likely to affect the central nervous system. His olfactory and aural imagery are well developed while his visual imagery is so weak as to be practically non-existent (except during his hypnagogic hallucinations which are predominantly visual and aural). Both parents are living and in good health; and neither has suffered from gross nervous or physical disorder. The subject's mother (who shows a normal E.E.G.) suffers from occasional migraine headaches (with 'fortification patterns') which are precipitated by disappointments. Both paternal grandparents were in good health up to the time of their deaths (due to cerebral haemorrhage in both cases). The maternal grandfather died of an intercurrent disease during an attack of delirium tremens; and the maternal grandmother developed senile psychosis (with paranoid symptoms) at about the age of 68. She died at the age of 74 from pneumonia.

The subject was examined on the electro-encephalograph at irregular intervals and at each examination he was given stimuli which were considered likely to influence the alpha rhythm. See § (*c*) below.

(*b*) *The sample*

The author has recently completed a small sample survey of forty normal individuals. In twenty-seven of the cases a mental arithmetic problem was set and the resulting alpha rhythm was measured in the same manner as for T.G.F. These cases are not a random selection from the total sample. Only those having a fairly continuous alpha rhythm could be used to assess the effect of the mental arithmetic problem (see § (*c*) below) on this rhythm.

(*c*) *Attention stimuli*

(i) *Mental arithmetic problem.* This type of stimulus has frequently been used as an inhibitor of the occipital alpha rhythm by other workers because of its reliability. In the present investigation, both with T.G.F. and with the normal sample, the majority of the problems set consisted of multiplying two numbers together, one of the numbers being between 11 and 19 and the other between 21 and 99. T.G.F. found that problems of this type required great effort and concentration of attention. He also reported that they were near the limit of his mental arithmetic ability.

(ii) *Concentration.* This task involves a purely internal stimulus in that, unlike the mental arithmetic problem, the subject sets himself the mental task. During the period of concentration the subject set himself an arithmetical problem of the type outlined in (i) above. The duration of the concentration period was decided by the experimenter who gave the instruction 'Concentrate!' followed by the instruction 'Relax!', after a suitable interval. The subject put his maximum effort into this task.

(iii) *Immediate memory span* (I.M.S.). In this test the subject was required to repeat after the experimenter a list of digits (between seven and twelve) read to him at the rate of about four digits per second. The procedure followed the usual form of testing immediate memory span; a full explanation will be found in any standard text-book of experimental psychology (e.g. Woodworth(8), p. 7). The lists of digits were approximately the maximum length that the subject could handle.

(iv) *Visual attention.* The effect on the rhythm of opening the eyes was recorded at each examination.

(*d*) *Emotional ('autonomic tension') stimuli*

The simplest method of arousing various forms of mild emotional tension (and presumably an associated autonomic activity) is by means of a word association test (cf. (11)). At each examination a list of stimulus words was read out, one by one, to the subject; and his response was followed in each case by an introspective assessment of the emotional response caused by the word.

IV. Measurement of results

(*a*) *Frequency measurement*

The alpha rhythm frequency was measured in cycles per second (c.p.s.) by means of a pair of dividers set to a length corresponding to a period of 1 sec. on the record. The number of waves between the points was counted with the aid of a watchmaker's eyeglass. Approximately twelve such frequency measurements were made of each record, and the mean and standard deviation calculated in each case. The standard deviation thus calculated gives a measure of frequency variation during the particular electro-encephalograph examination, while the standard deviation of the mean frequency measurements for all the examinations gives the frequency variation of the subject over a long period.

(*b*) *Measurement of suppression*

Precise quantitative measurements of amplitude changes in the E.E.G. traces are difficult to make owing to their irregular nature. Measurements of the degree of alpha rhythm suppression for all the types of stimuli mentioned were made by comparing visually the general level of activity during the stimulus with that occurring while the subject was relaxed with his eyes shut. They could be made only to the nearest 10%.

(*c*) *Artefacts*

The recognition of artefacts, such as those due to circulatory or respiratory changes, or to blinking, swallowing and smiling, comes with experience. Respiratory and circulatory artefacts—the former resulting from movement of the subject's head relative to the electrode wires and the latter due to setting the electrodes over the main arteries—can be eliminated by careful placing of the subject and the electrodes. Artefacts due to other muscular activity of the head were minimized by requesting the subject to relax all voluntary muscles and to avoid blinking and swallowing for as long as comfortably possible. The tendency to smile (a symptom of nervousness) passed off as the subject became adjusted to the general laboratory conditions. Those muscle artefacts which were recorded were easily recognizable by their relatively high frequency and spiky waveform.

V. Results

During the period March to September 1945, the subject was examined on sixty-three different occasions. Of this number, twenty-three traces have been rejected for various technical and psychological reasons. These examinations were made at all times of the day (up to as late as 1 a.m.) and irrespective of the subject's state of fatigue and mood. An introspective report from the subject shows that his attitude and feelings with regard to the experiment changed from an initial slight apprehension and a desire 'not to look a fool' when responding to the stimuli, to an attitude of indifference in which he found it difficult to remain awake.

Slight changes in the regularity of the alpha rhythm waveform were noted from time to time; but no connexion between this and the state of fatigue or mood of the subject has been found. On several occasions short irregular bursts of waves of higher than the dominant frequency have been noted. But the presence of these, too, seems to bear no relation to the subject's conscious feelings.

The results are given in Table 1 and the significance of the difference between the various means in Table 2. A single record is shown in Fig. 1.

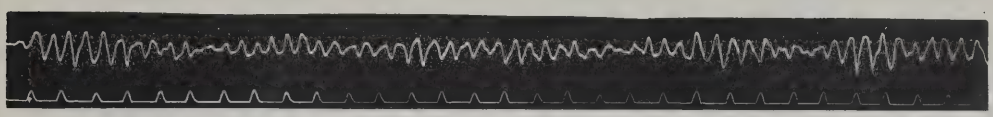

Fig. 1. Alpha rhythm of T.G.F. when relaxed with closed eyes. Time marks, $\frac{1}{5}$ sec.

VI. Discussion

(*a*) *General*

The constancy of the alpha rhythm frequency, both over the total period of the experiment and during the taking of any single record, is in good agreement with that found by other workers. Parr & Walter (1), Walter (2) and Jasper & Cruikshank (3), report small day-to-day variations in frequency, while the latter finds a variation of only 1·2–2·0 c.p.s. over a period of several months. It is interesting to note, however, that the rhythm takes an appreciable period to settle down to its constant value after it has been inhibited by visual activity. The extreme range of frequencies measured extended from 9·4 to 11·0 c.p.s., a range of 1·6 c.p.s. The greatest range noted in any single examination was 1·2 c.p.s. The changes in the dominant frequency were not found to be associated with any particular conscious state or feeling of the subject.

In the results produced by the non-visual tests the most prominent feature is the wide degree to which the amounts of suppression associated with the various stimuli are scattered about their means, and the marked asymmetry of the distributions. In no case is the standard deviation less than half the numerical value of its respective mean; and the individual measurements themselves may range almost anywhere between zero and about 80%.

Figs. 2 and 3 give frequency distributions for visual and other types of rhythm suppression respectively. The multi-modal appearance of the curves in Fig. 3 may be due in part to the method of assessing the degree of suppression (see § IV, p. 22); but without additional experimental work involving a larger number of examinations it is not possible to comment further on this point. There is no reliable indication of a decrease in suppression as a result of habituation to the experimental conditions or to the stimuli.

(*b*) *Attention stimuli*

(i) *Visual attention.* The suppression of rhythm associated with visual attention (opening the eyes and looking at any convenient object) is consistently greater, and shows less scatter, than that associated with any other type of stimulus used. This agrees with other results reported in other investigations on the electro-encephalograph.

Table 1. *Results*

(i) Frequency and alpha index

Mean frequency	10·1 c.p.s.	S.D.	0·187
Range	1·6 c.p.s.		

The frequency measurement at each examination was the mean of about twelve individual readings (see § IV, p. 22). The mean of the standard deviation of these means (that is, the frequency variation during a single examination) is given by *M* (S.D.).

M (S.D.)	0·22	S.D.	0·066
Range	1·2 c.p.s.		
Mean alpha index	90	S.D.	11·4

(ii) Attention stimuli

Mean suppression, visual activity	86%	S.D. 9·76
Mean suppression, mental arithmetic (sample survey)	43%	S.D. 35·60
Mean suppression, mental arithmetic (T.G.F.)	37%	S.D. 22·70
Mean suppression, concentration	27%	S.D. 21·70
Mean suppression, memory test	20%	S.D. 16·10
Correlations:		
Problem suppression and Concentration suppression	0·500*	P.E. ±0·095
Problem suppression and Memory suppression	0·200	P.E. ±0·120
Concentration suppression and Memory suppression	0·150	P.E. ±0·130
Problem suppression and Problem solution time	0·285	P.E. ±0·113
Memory suppression and no. of digits	−0·414	P.E. ±0·337

(iii) Emotional stimuli

	Emotional response		
	None	Average	Large
Mean suppression	27·50%	29·20%	30·60%
S.D.	24·40	26·40	26·10
Mean reaction time in seconds	13·10	16·30	17·50
S.D.	3·87	3·52	4·58
Number of words	130·00	61·00	17·00

Correlation, word suppression and reaction time −0·120* P.E. ±0·027

* Significant.

Table 2. *The significance of the differences between means*

Mean suppression due to	1	2	3	4	5	6	7	8
1. Problem	—	NS	S	NS	NS	S	S	NS
2. Concentration	—	—	NS	NS	NS	NS	S	—
3. Memory span test	—	—	—	S	S	S	S	—
4. Large emotional response	—	—	—	—	NS	NS	S	—
5. Average emotional response	—	—	—	—	—	NS	S	—
6. No emotional response	—	—	—	—	—	—	S	—
7. Visual attention	—	—	—	—	—	—	—	—
8. Problem; sample survey	—	—	—	—	—	—	—	—

Mean associative reaction time of words of	1	2	3
1. Large emotional response	—	NS	S
2. Average emotional response	—	—	S
3. No emotional response	—	—	—

NS, Not significant. S, Significance.

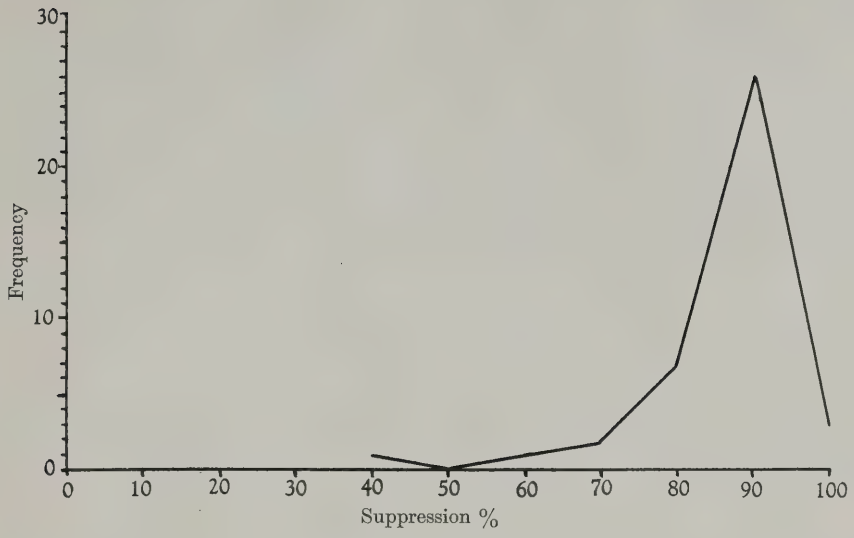

Fig. 2. Visual attention.

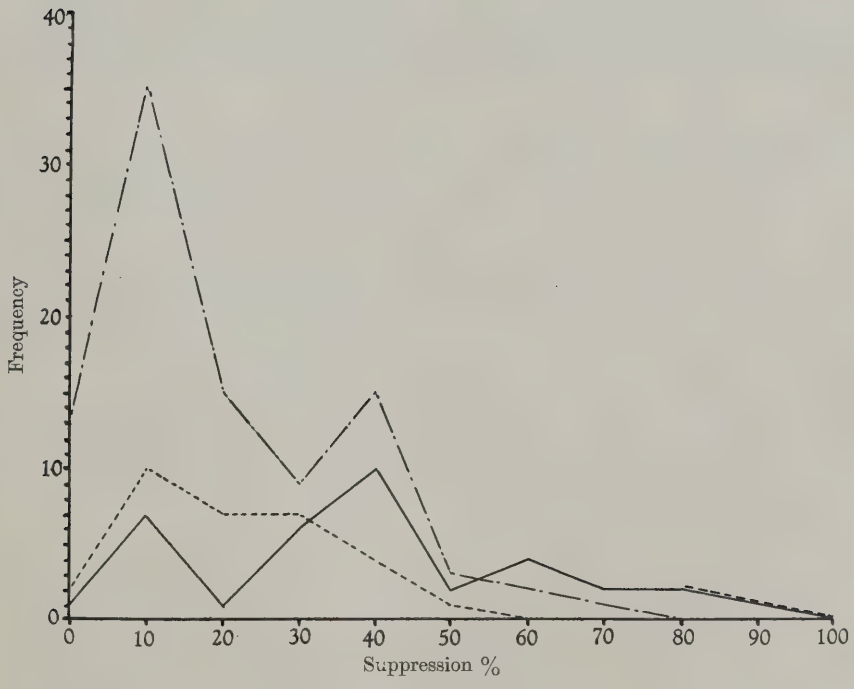

Fig. 3. Other attention stimuli.

——— = Problem. - - - - - = Concentrate. — · — · — = Memory span.

(ii) *Other attention stimuli.* Cyril Burt(4), Parr & Walter(1), Walter(2), Jasper(5), Jasper & Andrews(6), Jasper & Cruikshank(3) and others conclude that the state of concentrated attention is one of the major factors causing suppression of the occipital alpha rhythm and positively correlate the degree of suppression and concentration of attention. The mental arithmetic problems and immediate memory span tests were near the limit of the subject's mental abilities in these tasks and necessitated extreme concentration of attention. The subject stated that he worked at a similar level during the concentration periods. In spite of this high level of mental work necessitated by the stimuli (which, it would be expected, would be associated with consistently large suppressions of the rhythm) the degree of suppression varied widely from day to day.

Of the three types of attention stimuli the arithmetical problem produced a significantly greater mean suppression than the immediate memory span tests. There is no clearly significant difference between the results of the concentration test and those of the others. The general trend of the figures, however, indicates that the arithmetical problem produces the maximum suppression, the immediate memory span tests the minimum suppression, and concentration tests an intermediate value between these two (see Table 2).

As would be expected from the foregoing data, the inter-correlations of the attention stimuli are small and of the three calculated only one, the correlation between arithmetical problems and concentration tests, is significant. This correlation cannot be accounted for by the effect of practice or habituation to the experimental conditions, and must be attributed to the use of the same mental (or neural) function in the performance of the two very similar tasks. The lack of significant correlation between the tests of immediate memory span and those involving mental arithmetic or concentration seems to indicate the use of different mental (or neural) functions for memory processes, and to suggest the existence of different types of attention, each specific to a different mental process.

There is a small positive correlation of doubtful significance between the amount of suppression caused by a problem in mental arithmetic and the time taken for its solution. This correlation cannot be accounted for by practice or habituation; and appears to be due either to a positive correlation between the amount of suppression and the effort required to solve the problem of a given degree of difficulty (difficulty being measured by the time required to solve it), or to fluctuations in the subject's capacity for solving such sums (the longer the solution time, the lower his ability), which (one might suppose) is perhaps connected negatively with suppression of the alpha rhythm.

There is no significant correlation between the number of digits in the immediate memory span tests and the degree of suppression produced. It was noted that the subject's immediate memory span increased from about eight to about eleven digits during the course of the experiment.

As mentioned above, the test of mental arithmetic was the main task used with the group of twenty-seven persons. With these the degree of rhythm suppression was assessed in the same way; and the mean obtained from the group was similar to that obtained in the series of thirty-six successive experiments with the present subject, T.G.F. For the group the amount of inter-individual variation, as measured by the standard deviation, was 35·6; for T.G.F. the amount of intra-individual variation, measured in the same way, was 22·7 (see Table 1). As might be expected, the group variability was somewhat greater than the intra-individual variability; but the difference is so small as to be statistically

insignificant with a series of this size. It is, in fact, barely 1·46 times its standard error. Since the distributions are markedly skewed, and their variation in the direction of the zero-limit cannot be greatly increased, any rise in the variability is likely to be attended by a rise in the mean value. For the group of twenty-seven the mean amount of suppression was 43%; for T.G.F., 37% (Table 1); but once again the difference is statistically insignificant.

It appears, therefore, that the variation from one occasion to another within a single individual is large enough to account for the bulk of the variation from one individual to another. To assess more precisely the relative importance of the two factors a larger number of experiments with an analysis of variance would be desirable; and further investigations to study this problem have already been planned.

(*c*) *Emotional stimuli*

The varying degrees of emotion aroused by the stimulus words used in the association test were fairly easily classified by the subject. Responses causing practically no emotion (*N*) were described as being almost automatic, and were associated with very little conscious effort. These responses were easily distinguished from those causing an average (*A*) or large (*L*) degree of emotional reaction; but division into the two latter classes was found more difficult. A large emotional response was sometimes made apparent to the experimenter by various overflow phenomena, such as movements of the limbs, smiling, etc. Of the two hundred and eight words administered, one hundred and thirty caused no emotion, sixty-one average, and seventeen a large emotional response.

There is no significant difference between the mean values of suppression of the rhythm for the three types of emotional classification although the calculated figures give an indication that a large emotional response is accompanied by a greater degree of rhythm suppression. The scatter of results about their respective means is similar both for the attention stimuli and for the emotional stimuli, irrespective of the emotional classification of the latter (see Table 2).

These results confirm the lack of correlation between the E.E.G. and the psycho-galvanic reflex (P.G.R.) found by Forbes & Andrews (7). The latest conclusions regard the P.G.R. as an indicator of sympathetic activity ((8) and (9)); and the lack of correlation between this and the E.E.G., together with the lack of significance between the mean suppression for the three types of emotional classification, appear to rule out the E.E.G. as an indicator of activity of the sympathetic branch of the autonomic nervous system. Jasper (5), however, states that arousal of the autonomic system ('autonomic tension') is a prominent factor in the suppression of the alpha rhythm. This apparent contradiction is due, no doubt, to the use of the equivocal term 'autonomic tension'. Jasper & Cruikshank (3) associate suppression of the alpha rhythm with such emotional states as anxiety, worry, fear and general mental 'tension'. There is no evidence that the slight apprehension of T.G.F. at the beginning of the experimental series inhibited the rhythm, as the alpha index remained at more or less the same level throughout the series.

With regard to the association reaction times (R/T), there is a significant difference between the mean R/T for responses carrying no emotion and those accompanied by an average or large degree of emotion. This, of course, is in agreement with the work of Crossland (10), Jung (11) and Jung & Riklin (12). There is also a very small, but significant,

negative correlation between R/T and suppression. This may be due to a slight, but insignificant, increase in R/T and a slight, and again insignificant, decrease in suppression running throughout the results.

VII. Summary and conclusions

A single normal subject has been repeatedly examined on the E.E.G. and the effect of certain types of attention and emotional stimuli systematically examined. Inter-correlations of the stimuli are small and the scatter of the data about the respective means is large, with the exception of suppression of the alpha rhythm associated with visual activity. The effect of a mental arithmetic problem upon twenty-seven cases of a small sample of normal individuals has been compared with the results of the present experiment. The following conclusions may be tentatively drawn from the results. It should be emphasized that these conclusions are based on work with this subject only, and cannot be generalized without further individual investigations:

(i) There is a low positive correlation between the degree of suppression associated with mental arithmetic and concentration problems. This appears to indicate the existence of common mental (or neural) activities for these two tasks. The lack of correlation between these two tasks and the immediate memory span test suggests that the attention associated with immediate memory processes is to be distinguished from that involved in arithmetical problems and indicates specific types of attention.

(ii) Suppression of the alpha rhythm is associated with focusing of the attention, but is an unreliable guide to the degree of focusing. This indicates lack of direct connexion between attention and the amplitude of the alpha rhythm.

(iii) The amplitude of the alpha rhythm is an unreliable indicator of activity of the sympathetic branch of the autonomic nervous system.

(iv) Suppression of the alpha rhythm is not reliably connected with the degree of conscious emotion. The results indicate that a large emotional response is associated with a greater degree of rhythm suppression than either an average or small emotional response, but the difference between the means of these three values is not statistically significant. A small negative correlation exists between associative reaction times and degree of suppression.

(v) The neural processes directly responsible for suppression of the alpha rhythm take place below the level of consciousness since the subject is unable to state the degree of suppression associated with a particular stimulus.

(vi) In solving problems in mental arithmetic the variation in rhythm suppression shown by the single individual, who served as the main subject in this experiment, is almost as wide as that found in twenty-seven different subjects. With the latter both the standard deviation and the mean were slightly greater; but the increases were so small as to be statistically non-significant.

The experiment has been made possible by the use of apparatus generously loaned by Prof. Cyril Burt and Dr S. J. F. Philpott, and by their help and advice. The electroencephalograph was operated, and many of the results were calculated, by the author's wife, whose help was invaluable. The writer also wishes to thank Mr T. Driscoll, owner of the International Language Club, Croydon, for the good-natured tolerance of the use of one of his rooms as a laboratory.

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THE COLOUR SENSE OF FIJIAN NATIVES

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I. *Introduction* (p. 30). II. *Ethnic constitution of the groups investigated* (p. 30). III. *Method of testing* (pp. 30–31). IV. *Results of testing* (pp. 31–32). V. *Interpretation of the results* (pp. 32–33). VI. *Fijian colour terminology* (pp. 33–35). VII. *Colour in native decorative art* (p. 35). VIII. *Summary of results* (pp. 35–36). *References* (p. 36).

I. Introduction

This investigation was carried out during December, 1944, in Suva, the capital of the Colony of Fiji, where the writer was serving with the Fiji Military Force. It was intended to determine the validity of the frequently heard assertion that the ability of the average Fijian native to discriminate colour was less than that of the average European. As this assertion was founded mainly upon the achromatic nature of most native art and upon an apparent confusion in Fijian colour terminology, it was thought that an investigation of these two premisses might show the extent to which they justified the conclusion. The testing for colour blindness was subsidiary to the main study, but as the results proved interesting and possibly of wider significance, it was decided to extend the testing and to obtain, as far as military duties would allow, comparative figures for other ethnic groups, namely, Indians and whites. For the same reason this part of the study is given first place in the following paper.

II. Ethnic constitution of the groups investigated

Racially, although not culturally, the Fijians are predominantly Melanesian and thus basically negroid. Owing to the fact that the islands lie at the eastern extremity of Melanesia, there has been some mixture with the lighter-skinned, partially caucasoid Polynesians. The extent to which this has modified the Melanesian stock cannot be accurately determined, but the physical characteristics of hair type, pigmentation, nose shape and height are fairly reliable guides to ethnic classification. Throughout the investigation, therefore, attention was paid to these characteristics and, in doubtful cases, was supplemented by careful enquiry into genealogy. Such inquiry was often informative, as much of the mixture with Polynesians (mainly Tongans) has occurred in comparatively recent times.

The majority of the Indian group comprised immigrants or their descendants from the United Provinces or the Madras Presidency. Of the remainder practically all came from the Punjab or the Bombay Presidency. None had any admixture of European or Fijian strains.

III. Method of testing

The test employed was the shorter form of the Ishihara test comprising sixteen plates. It was administered in daylight by the writer to each subject separately. The Fijian language, in which the writer was competent, was used in testing the Fijians. In the case

of the Indians, English was generally used. In the very few cases where this was imperfectly understood, recourse was had to Hindustani by means of an interpreter, and the subject was asked to trace the figures on the cards with his finger. Each response was noted separately on a result card. Considerable care was taken to ensure that failure to respond correctly was not due to low visual acuity.

A number of subjects, mainly in the Indian group, gave a wrong response at the first reading, but were correct at a second attempt. This initial failure may have indicated a degree of 'colour weakness' in these subjects, but as no supplementary tests were available for more detailed investigation, they have not been included in the colour-blind category. The most frequent of these incorrect responses was the reading of card 7 as 71 or, more rarely, as 24, the correct response being 74.

The classification into photerythrous ('green-blind') and scoterythrous ('red-blind') types is based on the responses to cards 12 and 13. If the responses were 2 and 4 respectively, the subject was classed as photerythrous. If they were 6 and 2 respectively, the subject was classed as scoterythrous. In cases where the responses to both cards were correct, namely 26 and 42, but were accompanied by definite failure on several of the other cards, the subject was classified as incomplete red-green blind.

The Fijian males in the higher age group were all Army personnel. Those in the lower age group and all the Indian subjects were pupils of the Toorak Boys' School, Suva. The Fijian females were pupils of the Ballantyne Memorial School, Delanavesi.

IV. Results of testing

A. *Fijian males*

	No. in group	Red-green blind	Percentage red-green blind
Ages 18–35 years	387	2	0·5
Ages 6–14 years	221	3	1·4
Total	608	5	0·8

In the higher age group, one subject was photerythrous and one incomplete red-green blind.

In the lower age group, two were photerythrous and the other scoterythrous.

B. *Indian males*

	No. in group	Red-green blind	Percentage red-green blind
Ages 6–14 years	148	12	8·1

Only one age group was tested. Six subjects were photerythrous, three were scoterythrous and the remaining three were incomplete red-green blind.

C. *Fijian females*

	No. in group	Red-green blind	Percentage red-green blind
Ages 14–18 years	99	—	—

It is a common Fijian belief that women are poorer at colour discrimination than men, apparently because their terminology is often inaccurate. Probably this is due to their lower cultural level in Fijian society.

No tests of Indian females were carried out. Three subjects of mixed Polynesian and European ancestry were tested. One was incomplete red-green blind. Of twenty subjects

who were known to be of mixed Polynesian and Fijian descent, three were red-green blind.

In 1939 the writer supervised the testing with a selection of Ishihara cards of 2000 male European visitors to the New Zealand Centennial Exhibition in Wellington. Several investigators participated and the testing was carried out under artificial light. Hence the degree of reliability of the results is not as great as that of the results given above. The percentage of red-green blind subjects in the New Zealand group was 6%.

V. Interpretation of the results

The important feature of the results appears to be the difference in the frequency of red-green blindness between the Fijian and the Indian groups. The number of subjects seems sufficiently great to give the results a fair degree of reliability. The findings, too, are supported by other observations. The officer commanding the New Zealand Army Optical Unit stationed in Suva stated that he had examined approximately 200 Fijian subjects with the Ishihara test and had found none colour blind. Before the writer commenced the tests at the Toorak Boys' School, the Principal expressed the opinion that a high percentage of his Indian pupils were colour blind. He had come to this view from observing errors in the colouring of their drawings and from their confusion in colour terminology.

Various suggestions may be advanced in explanation of this difference between the groups.

(1) *Dietary factors.* Studies by Loken (1) and others have suggested that red-green blindness may be partially accounted for by deficiency of vitamin A.

No official nutritional surveys have yet been made in the Colony, and hence no accurate statement can be made regarding the amount of vitamin A in the average diet. There can be little doubt, however, that the diet of the Indians is deficient in this respect. Rice, their staple food, contains practically no vitamin A. A quantity of the vitamin would be supplied by the chillis and curries with which they season the rice, but this would be inadequate. The standard of living of the great majority of the Indians is low and their land holdings are small. Hence their diet of rice is supplemented only rarely by vegetables, fruit and meat. Fish, milk and ghee are consumed slightly more often, but scarcely frequently enough to supply an appreciable quantity of the vitamin.

The staple food of the Fijians is taro root, which is also low in vitamin A content. In the villages, and to a less degree in the towns, however, this article is supplemented by liberal quantities of green vegetables cooked in coconut juice, fish, fruit (including oranges) and occasionally by beef. Fijians, too, usually drink, or make soups from, the water in which such foods have been boiled. Thus, although it is possible that the amount of vitamin A in the Fijian diet is below the optimum amount, it is certainly greater than that in the Indian diet.

From this evidence alone, therefore, some support is gained for the theory that red-green blindness is to a certain extent dependent upon vitamin A deficiency. Possibly, too, the evidence might seem to be further strengthened by the fact that the percentage of red-green blindness was higher in the lower age group of Fijian subjects, all of whom were resident in the towns where diet is poorer. Considerably more experimental evidence, however, would seem to be required before such a theory could gain acceptance. It should be noted that Hamilton, Briggs & Butler found only a dubious improvement in the responses of colour-blinds to the Ishihara test under vitamin A administration (2). Such

a theory also would fail to account for the lower incidence of colour blindness in the Fijian as compared to the New Zealand group, since the diet of the latter is almost certainly the richer in vitamin A.

(2) *Ethnic factors.* It is interesting to compare the results of the present investigation with those obtained from studies of different racial groups in the United States. Clements (3) gives the results shown in Table 1 obtained with the Ishihara test.

Table 1

Group	Investigator	No. of cases	No. of colour blind	Percentage of colour blind
Whites	Von Planta	2000	159	7·95
Whites	Miles	1286	106	8·24
Whites	Haupt	448	35	7·81
American Indians	Clements	624	12	1·92
American Negroes	Clements	323	12	3·71

The interesting feature of this table is the relationship between the percentages for negroes and for whites. Although the individual percentages for both these groups are higher, possibly owing to a difference in the classification of colour blindness or to the relative age of the test cards employed, the relationship is similar to that between the Fijian and the New Zealand groups. This accords with the fact that in physical type the Fijians are basically negroid, although far removed in time and space from the African negroes and their American descendants. Thus some small support is gained for the view that there may be a racial difference in the incidence of colour blindness, the origin of which must be placed far back in human history. On this evidence alone, it would seem that a low incidence is a characteristic, distinguishing the negroid races from those of caucasoid stock, such as the East Indians and the Europeans. The low figure recorded for the American Indians, however, indicates that a low incidence is not confined to the negroid groups, and suggests that further studies are needed before the exact nature of any ethnic difference can be defined. One such study would be an investigation into the incidence of colour blindness in a purely Polynesian group such as the Samoans. As they are caucasoid rather more than negroid in origin, it might be expected that the percentage of red-green blindness in such a group would approximate more to that of the Europeans than to that of the Fijians.

The proposition of Vernon & Straker (reported in (4)) that colour-vision defect may be racially connected with pigmentation is not supported by our investigation.[1] The pigmentation of the Indian group was in general darker than that of the Fijian group.

VI. Fijian colour terminology

A common first impression of Fijian colour terminology is that it is limited and confused. As is the case among many other native races, the one term connotes both blue and green. The darker shades of every hue are called by the same term as black. 'Red'

[1] In the same issue of *Psychological Abstracts* is a review (68) of a paper by B. D. Karpinos (5), in which the writer states: "A higher proportion of colored selectees had normal vision in each eye than white selectees, irrespective of age. These differences in normal vision between white and colored groups showed no decrease with age and tended to be somewhat larger in the older than in the younger age group. It is possible that diminution of visual acuity with age is slower among colored than among white individuals. The racial differences in visual acuity persist on all levels of vision, with the result that colored subjects with imperfect vision were superior to whites with imperfect vision. There is no indication as to whether the differences are due to genetic, occupational, or other social factors." The Army Optical Unit has found a similar position in regard to Fijians and Europeans.

includes dun and brown, so that a native of lighter pigmentation than the average is called literally a 'redskin'. Investigation, however, shows that the limitation and confusion is more apparent than real.

A series of cards of various hues was submitted to three intelligent natives from the province of Bua, Vanua Levu island. They were required to state the native term for each colour. The following is a list of their responses:

Fijian	English	Fijian	English
vulavula	white	gaigaia	orange
loaloa	black	dromodromoa	yellow
dravadravua	grey	drokadroka	green
masiraki	brown	karakarawa	blue
damudamu	red	somosomota	violet
rerega	yellowish red	lokaloka	purple

Capell ((6), p. 205) lists in addition the following Bauan[1] terms for colours which were not represented among our cards:

Fijian	English
damu vakula	crimson
kulakula	scarlet
nionioro	sky blue

This nomenclature is supplemented by: (*a*) combination of terms to designate intermediate hues: e.g. a hue intermediate between red and yellow was designated by one subject as 'dromodromoa damudamu', i.e. 'yellow red'; (*b*) at least three terms to express degrees of brightness, e.g.:

Fijian	English
damudamu dina	red (of medium saturation)
damudamu sigasiga	light red
damudamu siasiawa	very light red

(*c*) a distinction between 'shiny' and 'mat' colours, e.g.:

Fijian	English
loaloa iloiloa	a 'glassy' black
loaloa (dina)	a 'mat' black
loaloa sigasiga	a greyish black

(*d*) comparison with familiar natural objects of the same colour.

It is interesting to note that all three subjects used the term 'drokadroka' for green, thus distinguishing this hue from blue, for which they used the term 'karakarawa'. Most Fijians will make this distinction when faced with the specific task of discriminating between blue and green. Normally, however, they apply the term 'karakarawa' indiscriminately to both hues. The word 'drokadroka' really means 'raw', 'fresh' or 'immature'. Foliage is always referred to as 'drokadroka' and never as 'karakarawa'. Thus it seems possible that the term in the sense of 'green' was originally applied only to foliage, and that its application was subsequently extended to provide a native term for the hitherto unclassified range of hues denoted by the English term 'green'.

Our evidence, therefore, does not support the view that Fijian colour terminology is defective. It is true that in common speech the same term is used for both blue and

[1] Bauan is the official dialect of the Colony and is now known generally throughout the Group. Certain of the terms used by the Buan subjects above are Bauan, and not Buan, in origin.

green. This, however, indicates little more than that the Fijian has a different classification of colours. "... With regard to hue alone, any division must to some extent be artificial; the spectrum is continuous, with no sharp line between one colour and the next, and a great many ways of dividing it are equally possible" ((7), p. 144). The Fijian system, too, lays greater stress on intensity and saturation than does the English system, which is based almost entirely upon hue. Thus a dark blue or a dark red may be called 'dark' in Fijian, whereas we might refer to these colours as 'blue' or 'red'. If further discrimination is required, the Fijian is as capable of discerning the hue as we are of discerning the intensity. The difference is merely one of emphasis upon one or other of the two qualities. As Klineberg says with reference to a similar position among certain American Indian tribes, "[They] use the same word for both dark green and dark blue, and a different word for both light green and light blue, so that when they are asked to match colours it is natural for them to group them in this manner. This is no more peculiar than that we should use the same word, *blue*, to designate such widely different sensory experiences as those presented by baby blue, Alice blue, powder blue, turquoise, peacock, cobalt, Prussian, royal, midnight and navy blue. We have made our classification entirely on the basis of hue, with no regard for intensity or saturation. Other groups have chosen to make their classification differently" (*op. cit.*).

VII. Colour in native decorative art

Most of the Fijian decorative productions, principally patterned mats, fans, pottery and bark cloth, are in black and white only or in black, white and a reddish brown. This lack of hue, however, need not imply defective colour discrimination or appreciation. It is dictated by lack of suitable permanent dyes. Even the clay used to produce the reddish stain is often difficult to obtain as it is found only in certain districts. Yet so much is at least some colouring desired that long trips by land and sea are often made to obtain it.

Coloured wools, when available, have now replaced the older grass borders on mats. At dances natives are fond of garlanding necks, wrists and ankles with forest greenery. Sometimes, too, red flowers are worn in the hair. In the towns, bright colours in clothing are frequently seen. In this latter respect, however, Fijians appear to have more sober tastes than many native races in a similar degree of contact with the West. Even when bright colours are equally available, they will often choose a white, black, grey or khaki. This may be due to an appreciation of the aesthetic quality of such shades developed by their almost achromatic art.

VIII. Summary of results

1. Tests of different ethnic groups by means of the Ishihara cards showed the following percentages of red-green blindness among males:

Fijians	0·8
East Indians	8·1
New Zealanders	6·0

2. No colour blindness was detected in a group of 99 Fijian female subjects.

3. Indian diet is deficient in vitamin A. So also may be the Fijian diet, but not to nearly the same extent. Such dietary deficiencies fail to account for the difference in incidence of red-green blindness, as the New Zealand group, with a higher incidence, has a diet superior in vitamin A content to that of the Fijian group.

4. Colour blindness cannot be dependent upon pigmentation, as the East Indian subjects were more highly pigmented, and the New Zealand subjects less highly pigmented, than the Fijian subjects.

5. The evidence tends to support the theory that there is an ethnic factor in the incidence of colour blindness. A low incidence appears to be a characteristic of the negroid races.

6. Fijian colour terminology is not defective. It is based upon a division of the spectrum different from that of the European system and places greater emphasis upon the qualities of intensity and saturation.

7. The achromatic nature of most Fijian decorative art is not the result of defective colour discrimination or lack of aesthetic appreciation of hue, but is dictated by an almost complete absence of suitable permanent dyes.

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PUBLICATIONS RECENTLY RECEIVED

The Psychology of Invention in the Mathematical Field. By JACQUES HADAMARD. Princeton University Press. (London: Humphrey Milford.) 1945. Pp. xiii + 143. 13*s.* 6*d.*

Productive Thinking. By MAX WERTHEIMER. New York: Harper Brothers. 1946. Pp. xi + 224. $3.00.

Here are two books of the utmost importance to psychologists everywhere. Each is the work of a man of international reputation, and each will enhance a renown already firmly established. Hadamard's book is the easier to read, and is in its way a quite beautiful piece of work, with lovely and simple illustrations, technical no doubt in character, but so expressed that anybody can understand them; and with delightful and apposite comments on poetry and great prose, on painting and music, and on sciences of the more experimental kind. Wertheimer's book is more difficult, and reads—as, indeed, it appears to be—more or less like notes for a series of lectures which need the personal expansion and exposition that only the lecturer himself could give.

On the face of the matter, Hadamard's discussion follows more familiar lines. It starts, as it was bound to do, with Henri Poincaré, and rightly returns to him again and again. True constructive thinking in the case of mathematics is exhibited as a function of some dominant, directive idea, which, though it need not to begin with be at all sharply defined, nevertheless makes the whole process totally different from any mere association or chance combination of mental elements. Then there may be, and usually is, some stage during which developments take place which cannot be fully expressed, but are still directional and selective, and in the end the discoverer makes his choice, from among alternative final methods or results 'imperatively governed by the sense of scientific beauty'. The scheme is not, perhaps, very strikingly original but every vital part of it is worked out with skill, with concise and fitting detail, and with a combination of brilliant quotation and fresh illustration. The general effect of the whole is of something new, and in a psychological sense, something remarkably penetrative and good. There are a great many illuminating side comments about intuition, about the use of images and words and about different sorts of mathematical minds. And for those who wish to pursue investigation themselves, there are the questions which Hadamard set to many of his friends and colleagues on the basis of the results of which his book was largely produced.

Although Wertheimer's plan of treatment has a wider scope, much of it also is concerned with mathematical thinking. As would be anticipated, his approach is more experimental, less exclusively introspective. He is deeply convinced that the human mind is honestly set towards productive work, towards sustained effort to find 'structural' and not merely 'piecemeal' truth. It is this preoccupation with structure which is the key to the discussion. And so he also, as indeed could have been predicted by anyone who knows anything about his many other remarkable contributions to psychology, is violently opposed to any hypothesis of chance discovery. Indeed for him the whole search seems to go on a very much more fully consciously directive level than for Hadamard. The terminology preferred is less familiar. We have to deal, always in the interests of structure, with 'gaps', trouble regions, disturbances, 'superficialities', with structural grouping and segregation and 'centring', with transposability, structural hierarchy and the separation of peripheral from fundamental features. There are so many processes that one gets a little bewildered among them all, and they do not always stand out clearly one from another. But the discussion is vital and must be read and re-read, and all the illustrations carefully studied if its contribution to the psychology of productive thinking is to be fairly assessed.

Both discussions are themselves constructive essays of a very high order of psychological effort. Each ought to become exceedingly fruitful and to be used as a starting point for a large amount of additional investigation. Wertheimer's will probably be of more service in the laboratory because his many suggestions are combined with indications for the use of at least some perfectly concrete experimental techniques. Hadamard's will have the greater appeal to the serious general reader and to the lover of penetrative analysis based largely upon self-observation. In one respect each essay leaves a huge almost unexplored field—the day-by-day thinking of the ordinary man. To the thinker this may appear as productive, though to other people it may seem to deal with rather trivial issues. Perhaps when somebody comes along who

finds out how to study every-day thinking in a controlled manner, it may turn out that neither Hadamard nor Wertheimer has caught and expressed the whole psychological truth of the matter.

Personality and Religion. By WILLIAM BROWN. London: University of London Press, Ltd. 1946. Pp. 195. 9*s.* 6*d.* net.

In writing this book, Dr William Brown has set himself a task of no small difficulty, namely, to integrate those findings of clinical psychologists which relate to the structure of the personality, and the theories of psychologists such as McDougall as to its development, with the philosophy of ethics and the fundamental beliefs of religion. Dr William Brown demonstrates the very different nature of these various approaches to the relation of religion to personality. And he is to be congratulated on making it quite clear that ideas and values which all civilizations have recognized as marking the highest point of man's mental development cannot be 'explained away' in terms of more primitive and infantile beliefs and ideas—stigmatizing an error to which many psychologists are prone. But he is no more able than previous writers to effect any satisfactory integration of the psychological, philosophical and religious approaches. Since even the nature of the dynamic processes constituting volition, and the development of the 'outer layers' of the structure of the personality, are so little understood, it is not surprising that psychology can do little more than speculate as to the nature of the 'inner ego', or why it appears to cleave so obstinately to values which seem to have no material or biological significance.

Dr William Brown has made a gallant attempt to clarify these issues, although to the psychologist his discussion of existing philosophical speculation does not always appear quite apposite. It is clear however that he has reached the point where psychological knowledge ends and faith must supervene.

Diagnostic Psychological Testing. By DAVID RAPAPORT, with the collaboration of MERTON GILL and ROY SCHAFER. Chicago: The Year Book Publishers, Inc. 1945. Vol. I, pp. xi+573. Vol. II, pp. xi+516. $6.50 per volume.

It is impossible to do justice to this immensely long and detailed book in a brief review; but anyone who is interested in diagnostic testing should study it carefully, and would find it most valuable. The aim of the book is to demonstrate how psychotics, pre-psychotics, neurotics, and various types of these, can be diagnosed and differentiated from 'normals' by means of their differential results on a battery of tests. In this study, the subjects of the tests were cases from the Menninger clinic chosen according to an independently made clinical diagnosis; together with a control group of 'normals', who were also interviewed clinically to discover any abnormal characteristics.

The tests used were: Wechsler's Bellevue Intelligence scale, and the Babcock test; the Sorting and Hanfmann-Kasanin tests of concept formation; the Word Association test; the Rorschach test; and the Thematic Apperception test. In all these, qualitative results and relationships of quantitative scores were given more importance than absolute numerical scores. Thus in the Bellevue scale, it was the scatter of scores, and particularly the differences between Verbal and Performance subtest scores, and between the Vocabulary and other subtest scores, which were shown to have the greatest significance. Again, in the Babcock test, patterns of impairment and efficiency were found to be more important than absolute scores. In the Sorting test, various forms of inadequate sorting were emphasized—use of too narrowly or too loosely defined groups, inability to use the more abstract conceptual levels, etc. In the Rorschach test, some of the more recondite details of interpretation were dispensed with, but the verbalization, i.e. the exact verbal expression of the responses, was found to be diagnostically significant particularly for psychotics. In the Word Association test the most useful information was obtained from the formal characteristics of the associative reactions rather than from their content—i.e. whether the association was too close (repetitions, clang reactions, etc.) or too distant (highly idiosyncratic connexion); and from failures and delayed reactions. The Thematic Apperception test might give useful indications as to the nature of the patient's disorder, and also as to the contents of his ideas, in failure to obey the test instructions, incorrect perception of pictures, distorted verbalization, and bizarre, incoherent, over-emotional contents, etc., of the responses.

Careful study of this book does demonstrate how the various test results can be fitted together to diagnose various pathological categories. It would be rather easier to understand if a clearer summary of results were given at the end of the book. Even more valuable perhaps would be some indication of how these test results could be used to study the more or less 'normal' personality; but that no doubt would constitute another major study in itself.

Neurosis and the Mental Health Services. By C. P. Blacker. London: Humphrey Milford, Oxford Medical Publications. 1946. Pp. xxii+218. 21*s.* net.

This admirable book consists of a detailed survey of most of the mental health services of this country. It was carried out in 1942–3 by groups of investigators working in the eleven Civil Defence regions of England and Wales, in the main by means of questionnaires sent out to the psychiatric clinics. The information was centrally coordinated at the Ministry of Health by Dr Blacker, who also visited many of the regions personally. Beginning as a survey of the condition of the psychiatric out-patient services and their reaction to the war, it was extended to cover a number of wider problems: The present distribution of neurosis in the country, its relation to social, war and factory conditions, and the possible effect on it of post-war circumstances; the origins and distribution of the psychiatric services in general, and recommendations as to their future development; the disposal of mental and mentally defective cases; psychiatric services for children in particular, and their relation to the education services and the Education Act; the training of psychiatrists, mental nurses, medical officers of mental health and accessory services (psychologists, psychiatric social workers and occupational therapists). Incidentally, little is said of the types of psychiatric therapeutic treatment in use, except that (*a*) they appear to be very varied, and (*b*) the general public is apt to view them with suspicion. It would be interesting to have a parallel survey of these methods, and some recommendations as to their future.

It will be of particular interest to psychologists to note that Dr Blacker suggests that the psychiatric services for children should be divided into Child Guidance Centres and Child Psychiatric Clinics. The former should be "the headquarters of a psychologist working on an area basis" in close conjunction with the schools and education services. But the supply of psychologists is quite inadequate to meet the probable demand; here is "the most serious bottle-neck of any that affects the essential and accessory services concerned with mental health". Facilities for training are nowhere sufficiently extensive, "but...could be expanded if financial help were made available". An official investigation should be made into the demand, supply and training of the accessory services.

The Origin of Food Habits. By H. D. Renner. London: Faber and Faber, Ltd. 1944. Pp. 261. 15*s.* 0*d.*

This book contains much to interest the student of psychology, whether he is himself much concerned with food or not. The author's primary approach is the psychological one, though facts—most of them very well selected and authenticated—mingle in a somewhat higgledy-piggledy manner with theories. There are four parts: *Psychology versus Dietetics*; *Agriculture, Climate and Preserving*; *Introducing Manufacturing Technique*; *Sociological and Historical Factors.* The first section is the longest and many readers, especially if they prefer facts to dogma, may find it the most interesting. It opens up many problems which the author bravely tries to solve decisively, but which the more careful reader will desire to submit to further experiment. For example there are some very good, sound remarks about sensory fatigue, many of which have received less consideration than they deserve. But Mr Renner rather tends to jump to immediate conclusions about the functions of sensory fatigue in relation to widespread habits of dealing with particular kinds of food. The whole book, this section as well as the other ones, is written in a jumpy sort of style, and is difficult to read right through, but very good to dip into. On the whole the least satisfactory section is the last one. The individual example is here made to carry too much weight, and evidence is least satisfactory just where it is perhaps most needed. Whether any reader will finish with a clear idea of "The Origin of Food Habits" is doubtful, but he will probably enjoy his reading, certainly learn a lot on the way, and be presented with much, and palatable, food for thought.

The Feminine Character. By Viola Klein. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1946. Pp. xv+228. 12*s.* 6*d.* net.

There is an age-long controversy as to whether there do exist innate psychological differences between men and women, or whether such differences as appear in our society are determined purely by cultural and environmental differences. Dr Klein gives an interesting discussion of some of the main theories and observations in this field; particularly those of Havelock Ellis, Weinberger, Freud, Terman and Miles, the Vaertings, Margaret Mead and W. I. Thomas. Dr Klein shows that even those who consider that there are innate differences between the two sexes seldom agree as to what these differences may be; although traits which are cited with comparative frequency as characteristic of women are passivity, emotionality, lack of abstract interests, greater intensity of personal relationships and an instinctive tenderness for babies.

She does not go as far as the Vaertings and Margaret Mead as to consider that any of these traits can appear with equal prominence in men if the cultural and social structures are favourable. But she agrees with Thomas that in many societies, and pre-eminently in western civilization, these traits may be the outcome of women's naturally greater passivity and endurance; their attachment to children, the home and home industries; their consequent subordination to man, the adventurer and inventor; and, in the last century, the complete dominance of middle- and upper-class women by the desire for marriage because male society denied them other functions or interests. In the modern period of transition, these latter restrictions have been considerably relaxed as regards the legal, occupational and social status of women, but not to the same extent as regards psychological interests and attitudes. Thus the effects on creative work in women of social discouragement are still persistent and widespread.

Without arriving at any startlingly original conclusions, Dr Klein has nevertheless selected, compared and discussed in an interesting and readable manner a large body of material. And her findings from this should be encouraging as regards the future readjustment of men's and women's capacities to the needs of a changing society.

Dictatorship and the Political Police. By E. K. Bramstedt. London: Kegan Paul, Trench, Trubner and Co., Ltd. 1945. Pp. x+275. 15*s*. net.

This book consists of a detailed and circumstantial description of the origin, functions and methods of the Secret Police, from its earlier examples in the Political Police of Napoleon I and Napoleon III, to the Italian OVRA and the German S.S. and Gestapo. Discussion of the Russian secret police is, perhaps tactfully, omitted. For the successful functioning of the secret police, some other backing by a strong army and by the use of propaganda is shown to be necessary. Even then, control by the secret police is not always successful, as shown by the resistance movements in occupied Europe and by the passive resisters of Norway; and it never continues to be successful over long periods of time.

Those who are interested in studying the factual detail relating to the operations of the secret police will find this book interesting and valuable; but little is said as to the psychological basis of their methods and their power, or of what enables people to resist them.

Aesthetic Experience and its Presuppositions. By Milton C. Nahm. New York and London: Harper Brothers. 1946. Pp. xiii+554. $4.50.

A philosophical discussion of the nature of the aesthetic experience and its relationship to the psychological nature of 'feeling'.